New Archaeology

its scope and application to India

New Archaeology its scope and application to India

D. N. Majumdar Lectures 1974



H. D. Sankalia
Professor Emeritus
Deccan College, Poona

Ethnographic and Folk Culture Society
Lucknow (India)

Price Rs. 28.00 U.S. $ 8.00

Published by Ethnographic and Folk Culture Society,
Post Box 209, Mahanagar, Lucknow (India).

Printed at Pnar Mudrak, 117, Nazirabad, Lucknow (India)

Author's Preface

Three years ago in August 1973 when I was on my way to Montreal to attend the IX International Congress of Anthropological and Ethnological Sciences at Chicago, Dr. K. S. Mathur met me in the plane taking us to New York and asked me whether I would deliver the Dr. D. N. Majumdar Lectures on 24-25 December 1974. I readily agreed, for it was an honour for me to associate myself with the memory of the great anthropologist. At this time the subject of New Archaeology was uppermost in my mind, so I also told him the topic on which I would deliver the lectures.

I was anxious not only to profit by whatever new concepts and new techniques that were advocated in the New World, but to digest them and use them in India, instead of getting enamoured of whatever is new without understanding and knowing its scope in India. This has necessitated a critical and exhaustive appraisal of the new perspectives in archaeology, perhaps the first one by a non-American. Incidentally, the problems of subjectivity and purely new scientific approach in archaeology have also been touched. The former is also important, because as so many of us in India and Europe know, all archaeology is not prehistory. The latter is a part of a vast subject. And even for a proper appreciation of monuments of the historical archaeology, written accounts—texts—are of great value. What Professor Christopher Hawkes had said about a palatial establishment of King Edwin of Northumbria is true of so many monuments in India, such as Moghul palaces at Agra and Fatehpur Sikri, and earlier ones at Mandu. Even

if they had been carefully excavated with all the contextual things, as the American Indian Pueblos are, we would not have known the real and full purpose of them at Fatehpur Sikri, but for the detailed description in *Aini-Akbari*, and in the accounts of foreign travellers. For, slightly earlier ruins at Champaner, so carefully exposed by Dr. R. N. Mehta and Dr. S. N. Chowdhury and their colleagues of the Maharaja Sayajirao University of Baroda, defy full explanation in the absence of any contextual remains and written accounts !! Thus in a country like India all possible means, conceptual as well as others, should have a full play. This I have tried to point out with examples in these lectures. There being only two, some limit has been placed on my treatment. It could not be exhaustive nor very much detailed. Though such memorial lectures are not usually fully documented, I have done so, looking to the controversial nature as well as "newness" of the subject in India.

Acknowledgements

For allowing me to reproduce illustrations from their works, I am thankful to Professors R. J. C. Atkinson, J. G. D. Clark, and Dr. (Mrs.) Jo Patty Watson, and the publishers of their scholarly works.

I am also thankful to my colleagues Dr. V. N. Misra and Dr. K. Paddayya of Deccan College, Poona for going through the text of the lectures before they were delivered.

I must also thank Dr. K. S. Mathur, Dr. B. R. K. Shukla and their colleagues for their hospitality when I was at Lucknow, and I must thank the Ethnographic & Folk Culture Society, U. P. for giving me this opportunity to pay my tribute to the memory of Dr. D. N. Majumdar, and for promptly arranging the publication of these lectures. I am particularly thankful to these friends for going through the proofs, and seeing that the lectures are published, facing all the difficulties which a badly typed and heavily corrected manuscript presents.

Contents

List of Illustrations

Lecture I

New Trends

After the termination of the Second World War, among the various new trends of thinking, development of new techniques and new methods, particularly in science, there were suggestions at first, later actual demands, that the aim in archaeology should not be merely a collection of facts of the past, but the aim should be interpretation and reconstruction of the past. For these—interpretation and reconstruction–resort should be had to sciences—physics, chemistry, mathematics, biology–zoology, botany, and astronomy. With the result that not only the beautiful art objects, but even the most ordinary things like bone, ash, and samples of earth and soils from excavation or otherwise were studied, from various points of view. Within a few years we had a few outstanding books

like *Science in Archaeology* by Brothwell and Higgs (1970). This was certainly a most welcome step, because we were beginning to understand not only the cultural history but environmental history of the past–historic, proto-historic and prehistoric.

Old views challenged

But very soon, particularly during the last 10-15 years, a few scholars–specially Americans–began to express dissatisfaction even with these advances in archaeology. Trained as they were in anthropology and not classics, like the British, Europeans and Indians, these young American archaeologists began to question the theories and assumptions that archaeologists and culture-historians like Braidwood (1960) and Gordon Childe (1944, 1954, 1956) and several others had made during the last 50 years.

Archaeology & Prehistory

It may be recalled that one of the fundamental differences between the "old" and the "new" archaeologists is that in the Old World, archaeology developed as a branch of history and classical studies–Greek and Latin in Europe, and Sanskrit and Prakrit in India. In America, on the contrary, archaeology has been regarded and studied as a branch of anthropology. Secondly, in the latter, archaeology is generally or largely understood as a study of prehistory. This was but natural. In America, to the colonists everything before their arrival in 16th-17th century A. D. was prehistory, particulary because the Aztecs and other Red Indian tribes had no written literature, and their magnificent monuments had little significance to the colonists. In the Old World, on the contrary, besides writing there were intact buildings and ruins, even cities, whose antiquity stretched for at least two thousand years and more. And later when ancient sites in Iran, Iraq, Turkey and India were excavated, their antiquity was stretched much farther back. Thus there were reasons for the "envionmental differences" between the Old and the New World, archaeologists.

The scope of Archaeology in India–not Prehistory alone

In spite of some such reasons, we cannot restrict the scope and definition of archaeology to include purely prehistoric objects, or prehistory, that is the vast illimitable period stretching back to dim past, during which time there was no writing of any kind. Particularly this would be true of India, where by official definition everything over 100 years old (before 1950) comes under the perview of the Archaeological Survey of India. However, we shall not go into further discussion of this fundamental difference, but only consider the views of the new archaeologists as far as they can help solve our problems in India, because it is quite apparent that their views are quite irrelevant to such known archaeological monuments as Buddhist caves, Hindu temples and Muslim mosques, though even in this field there is some scope for socio-archaeological studies, as pointed out by me elsewhere (Sankalia 1966b).

Confining our attention or discussion to pure prehistory, let us briefly see what the main objections of the new archaeologists are, to what has been done and thought so far, and what steps, methods, techniques, and theories, hypothesis or models–they propose to make archaeology a science, or a more reliable branch of social sciences.

Brief Summary of Prehistory

Prehistoric studies are now nearly 150 years old, the first discovery of stone tools in the old bed of the Somme river having been made by Boucher des Perthes in 1839. Later more and more tools as well as associated animal remains began to be discovered. Later when some human remains like skulls were found at Neanderthal, Swanscombe and other sites in Europe and later in China and Africa, theories of human cultures associated with particular types or race of man began to be developed. The occurrence of artifacts of stone alone, of stone and copper and then of iron led to the formulation of the now famous Three and Four Age theory of Human Cultural

Development.[1]

Improvements or refinements in these chronologies were suggested, the most important being Gordon Childe's that the transition from the pure Stone Age cultural stage to the stage when stone artifacts were ground or polished was not a simple adoption of a new and improved technique in making stone tools, but it was indeed a "Revolution" in man's way of life, imply as it did, the gradual giving up of nomadism, the domestication of animals, and the beginning of agriculture, thus paving the way for surplus food, and the growth of arts and crafts. Gordon Childe and many other scholars also felt, dazzled as they all were by the high state of civilization compared to what they knew of Europe then that Egypt or the Near East (Western Asia) was probably the main or original centre from which civilization spread east and west. Acting on this theory of Breasted, and Childe, Braidwood sought to find this site in Western Asia where the ancestors of the domestic old species of grain and animals could have originated. And when he discovered Jarmo, Childe thought that Braidwood had performed the miracle and pleased with this assurance from the Grand Old Man of archaeolological synthesis, Braidwood wrote several articles and books, illustrating with charts how the various ingredients which go to make up civilization gradually grew and spread like eddies in a pool of water. These he has beautifully explained in the museum of the Oriental Institute at Chicago.

During the last 25 years, all these hypotheses have been questioned, and some are regarded as pure speculation (Higgs and Jarman 1969). Improvement or modification in the old theory or a completely different theory has been formulated on :

(i) the strength of new evidence, either from

(a) the nuclear area, or

1. These developments have not been given here in a chronological way. The statement here only highlights the developments. For a detailed summary see Sankalia (1962-63, 1974b),

(b) the areas regarded as backward such as Europe, S. E. Asia and S. E. Europe.

(ii) new knowledge based on the scientific study of the old evidence e. g. copper and bronze objects by spectrography, animal bones and study of pollen grains. (*e. g.*, Renfrew 1969)

(iii) the emphasis on the more complete and systematic gathering of evidence, so that statistics and computers could be used and human bias could be eliminated or reduced to a minimum.

(iv) the advancement of the view that so far archaeology has remained an immature and inexact discipline, depending upon personal prejudices, likes and dislikes, and with a view to improving it use must be made of

(a) Statistics and

(b) Computers

(v) and above all the archaeologist should not only employ the new scientific gadgets for the collection of his data, but he should formulate models or theories, and ask questions and seek help from ethnography/ethnology for understanding the behaviour of the people and even these should be tested. Citing of ethnographical parallels is not enough (Binford in Binford and Binford 1970 : 13)

Here entire or all the views are considered, and not only the evidence of the New Archaeologists.

Evaluation of all new views

(1) Some 50 years ago, the names of stone tools and the objects from French sites such as Aurignacian, Levalloisian were regarded as culture-chronological terms, which could be applied anywhere in the world, where tools/industries similar to these sites were found. This view is now completely given up and if terms like Acheulian and Levalloisian are used, these are used in a strictly technological sense, and not chronological.

(2) The Three or Four Age System has been severly criticised (Daniel 1943, 1950), still it has been found quite useful for arranging the developmental stages in India, Western Asia, and even Europe (See *Cambridge Ancient History* 1967; Jarman 1971; and Sankalia 1974). Though one would like to agree with Braidwood and use the descriptive term such as "Incipient Agricultural Stage", etc. Still there are two main objections to their usage. Firstly, the terms are too long for regular and constant use in a text book. Secondly, stages are not easily traceable everywhere, the data being absent or unavailable for various reasons. Thirdly, as pointed out by Higgs and Jarman (1969), we have no positive data for the domestication of animals and plants, as previously supposed by several palaebotanists and palaeontologists, and archaeologists. Hence, there is a suggestion to retain the use of the term "Neolithic" in its technical sense (Jarman 1971). I have also found that in a country like India, where these ground and polished stones occur in a particular context, or where the burins and such blade tools occur at a particular site, each singly or collectively helps us to designate the industry as Upper Palaeolithic and the other Neolithic (Sankalia 1974a)

(3) The 19th and early 20th century theories of Invasion and only one centre for the birth of man/civilization are being given up for two main reasons.

(i) More and more sites bearing traces of early cultures are being found in Western Asia.

(ii) C–14 dates from Europe suggest that the beginnings of agriculture and domestication of animals in this continent were not so late as previously supposed on a mere typological study of pottery and other objects; in some cases, these are earlier than those in Western Asia. (e. g., Ucko *et al.* 1969).

(iii) In the countries believed to have been influenced by the European or Western Asiatic culture, very early cultures have been found (Clark 1966, Gorman 1971)

(iv) It has been shown that these objects which were supposed to have been brought by Invasion or Colonization were received by trade, for which there was no evidence (Adams 1968).

(v) Thus a careful reconsideration of the entire evidence, and all cases was desirable.

(vi) It has been urged, and rightly that the excavations should be large–horizontal or areawise–and not only vertical and that everything should be more carefully observed and collected. Contextual data was indeed necessary.

(vii) Then only archaeaologists should/would be able to formulate general principles which would explain the occurrence of this or that event.

(viii) Archaeologist can recover all that has been buried and should be able to reconstruct a complete picture of the past that is the site he has excavated (Binford and Binford 1970).

These eight or more criticisms of earlier developments as mentioned above fall into three main groups. The first two are natural to any developing discipline. As new knowledge is obtained and methods refined or improved, the old views, theories, like that of Relatively, need a partial or total modification and even rejection. However, what the proponents of so-calld "new" archaeology want is to convert archaeology into an exact science or discipline, having its own "philosophy" (Watson *et al.* 1971)

Let us first see what their criticism of the old theories end practices is.

More exact collection of data

It has been urged by several palaeontologists that not only all the animal bones from an excavated site should be collected

and studied but these should be collected housewise, if possible, and then arranged periodwise if the site belongs to two or three periods or phases. It would then be possible to infer whether the inhabitants, say of house X ate more substantive protein food, because they ate this animal or that, that they were rich or poor, because the bones found in their house belonged to this or that part of the animal skeleton and further whether the animal was young or old, and domesticated or not. Thus the relation of bone groups to the excavated areas may reveal cultural or social diversity. A periodwise study of animal bones would also throw light on the man-animal relationship (symbiosis). It is even suggested that the palaeontologist should not only be able to identify the animal slaughtered for food from the excavated remains, calculate the protein content of the bones gathered from the house at each level/phase, but should be able to judge the size of the animal (Uerpmann 1973, 307-422). This might be possible, it is argued, if bones from certain parts of the body, such as skull, metapodial/and long bones can be had for study. The archaeologists/zoologists might also try to reason why only certain bones are available. In doing all this, of course, physical changes, particularly increase in weight of the bones due to calcification and mineralization should be taken into account.

How the identification of bird or animal bones helps to throw light on the past climatic conditions and the environment-animal world–has become common place. Instances will be cited later how these have advanced our knowldge in India (Zeuner 1963a; Sankalia 1965).

Not only bones, charred grains, and such other evidence now valued for knowing the diet of Early Man and his successors, but even the lavatories of ancient man. For such studies shed light on both the diet and the health of primitive people (Brothwell and Brothwell 1969).

Relative dating

Likewise, the study of the fluorine, nitrogen and uranium contents of animal and human bones from excavations has been

utilized to get a relative dating of excavated sites and other associated cultural remains. This is again a purely scientific approach to archaeological problems and Shri S. B. Lal, Archaeological Chemist, DehraDun has been advised to take up this problem on a regional basis in India, so that within a few years we shall have a parallel timetable, of course of a relative nature, for dating sites in this country. How this study exposed the fraud about the Piltdown Man is well known. Later the Piltdown bones have been dated by the radiocarbon method, when the collagen (gelatin) extracted from these bones by Oakley was determined at Groningen (Oakley 1959).

Spectographic studies

Another scientific aid useful not only for understanding the technique of manufacture of metal and other objects, but also useful for the light it throws on the mechanism of trade between distant countries, is spectography. This is a useful scientific aid and how profitably it can be employed in India has been mentioned in the new edition of my book *Prehistory and Protohistory of India and Pakistan.* (Sankalia 1974a). Now besides these, the University of Michigan has given us the results of extensive X-ray tests on Egyptian mummies. Such a kind of study which can rarely be undertaken by an individual or an ordinary institution has thrown light not only on the diseases which the Pharaohs and other Egyptians suffered from but also on the relationship between the members of a family, cultural

Goodall observed wild chimpanzees in the Gombe stream Reserve in Tanzania making simple tools, such as trimming a blade of swordgrass and using it to 'fish' in termite pests. (Goodall 1964, 1967).

Function of Stone Tools

With regard to the use/function of Early Palaeolithic tools, such as the most commonly occurring handaxes and cleavers, choppers and chopping tools, it has now been admitted by several scholors that while the actual use (as by the writer and the late Dr. L. S. B. Leakey) might indicate the probable function of these tools for digging out roots, chopping meat and breaking bones, the exact function is impossible to determine. This is for two reasons : (1) the tools are not used by any of the primitive tribe in Africa or Australia. A small primitive tribe called "Tasadays" very recently discovered in a cave at Mindanao, in the Phillipines use pebbles, stone scrapers, and sharpened bamboo knives for smashing and cutting, and do not look on their tools as weapons." (Macleish 1972).

Semenov too did not examine such larger tools. Since then the fashion has not only been to measure the three sides of a specimen, but a detailed account of the cutting edge, viz. the method of manufacture, the angle and shape, etc. And this is done to get over preconceived notions regarding the likely functions of the tools in question. All these details are then fed to a computer. While on this it would be relevant to cite the complexities of the problem discussed by Freeman (1972), and also recognized to a great extent by Dr. Kellar's committee which had met to discuss the problems connected with the measurements of stone artifacts.

All this is good, but the writer feels after handling thousands of tools from India, and a few from Europe and Africa that not only it is time consuming but adds little to our real knowledge of the function of the tool. As Professor Bordes has said "our mind is a computer. Years of practice enables it to pick out the essential basic similarities, and so this elaborate

work is indeed unnecessary." And, as David Clarke, the great champion of New Archaeology (including the use of statistics) has said that the only valid preliminary step in classification is the arbitrary separation of the relevant system of essential attributes from the inessential attribute system of the same entity .....and even then the key attributes of one population may not be assumed to be the key attributes of another population". (Clarke 1968 : 142). Of course this has not been appreciated by his other colleagues.

Along with this should also be mentioned the recent attempts to understand the manufacture of Early Palaeolithic tools, again by noting very carefully the nature of the hammers, and by a study—counting, analysis and photographing—of all the flakes and chips that come out from such an experiment.

Semenov largely worked on smaller tools–flakes and blades of the Middle and Upper Palaeolithic periods.

A careful observance with the microscope might reveal the direction from which the tool-flake blade-was used, and then determine its exact nature whether it was used for scraping or cutting, boring a hole, but such experiments again cannot be carried out on all assemblages.

We are thus reduced once again to our normal procedures viz. of classifying the assemblage according to the nature of the cutting edge, size and form. Here, I would draw attention to my experiment with the actual use of the blades from Navdatoli (Sankalia 1967).

Nothing more, it appears, can be done regarding the function of the Early Palaeolithic and Middle Palaeolithic tools, for these are far removed in time from us. But no longer it is believed that Early Man used only larger tools such as choppers and handaxes, because in the earliest assemblage from the Olduvai Gorge, Mrs. M. D. Leakey and Dr. Isaac have now discovered such small tools as scrapers, and even burin-like pieces. (Leakey 1973). The recent evidence from Australia suggests that some 25000 years ago man used scrapers of various kinds and these remained in use for a long time !! (Bowler *et al.* 1970).

The variability in artifact assemblages—
What it connotes

As it is now well known all the early collection of stone tools in Europe was of a selective nature. Somebody would only pick up from a site like Chelles only handaxes, another visitor/collector some other type of tool. And thus started the chronological and cultural inferences based on typology. This affected also the later stratigraphic collection giving rise to the famous division of the Acheulian by Breuil into Early, Middle and Late. Even at this time the desirability of making use total collections or collectors of all tools type was not felt and there was room for personal prejudices. After the Second World War, the renewed excavations in the French caves and rock-shelters were not only much more careful and carried out by trained excavators but the collections of assemblages were total and followed by detailed analysis of tool types.

When, therefore, sites of the Mousterian culture dug by Professor Bordes and others in France did not yield identical tool types at different sites and there were differences–stylistic–from layer to layer, even at one and the same site, the problem arose as how to explain this variation between the principal Mousterian sites. Bordes and Mrs. Bordes (who had earlier carried out a detailed study of all the Mousterian industries) explained these differences as due to the fact that probably there were different groups or tribes of people and hence the differences in types (Sonneville, Bordes and Perrot, 1953).

L. R. and Sally Binford, two of the most ardent champions of new archaeology argued that variation in the industry was due not to the existence of different kinds of peoples, but because one and the same people might behave differently owing to environmental-climatic and other reasons. For instance, the tools required for various purposes in the summer, or in an Interglacial Period might be different, from those required in the winter, or in a Glacial Period. Thus arose the "Behaviouralist School of thought". (Binford and Binford 1966).

Such a point of view is not quite wrong or inappropriate,

when we begin to view the stone tools or any archaeological object in a larger perspective. The question is and will always be "Do the people change their tool kit (as they might or have to change their food, clothing and even wives) owing to marked changes in the climate ?". There is no doubt that such ideas or points of view are due to our present frequent or seasonal changes in clothing and food. And there is some support from ethnographic literature as well as archaeological examples. For instance, a careful excavation of Starr Car, a site of the Mesolithic Period in England, by Professor J. D. Clark gave definite evidence (Clark 1954, 1972).

At Star Carr it was possible to prove stratigraphically that the site was occupied twice. Secondly, by the study of animal remains, particularly antlers of the red deer, it was also shown that the main period of settlement was the winter, during which the stags carried their antlers. This is a time of the year when herbivorous animals sought shelter on low lying ground. Again this is corroborated by the artifactual evidence—the number and nature of bone tools, tools on antler and microliths. Thus the broad conclusion that the site was occupied by a small hunting band which supported itself during the winter by the cutting of red deer stags, supplemented by hunting elk, aurochs and other game, and which took advantage of period of settled existence to replenish the equipment needed during the course of the year" (Clark 1972 : 10-31).

Similar evidence of seasonal occupation was previously obtained at several sites in Europe, particularly Holland and Denmark. Again, Sally Binford had shown by her study of the stone industries in Palestine that basic differences in tool types in a particular region might have been possibly due to environmental difference, combined with physical and social differences. (Binford and Binford 1970 : 56). The Behaviouralists, therefore, deserve a careful hearing. However, as far as the Mousterian Culture in France is concerned, Bordes are not inclined to accept the Binfords view. In their recent article they maintain that such a variability or change in tool assem-

blages might be explained as due to the arrival of a new set of people, or people who have been culturally different (Borde and Borde 1970 : 73).

During the last 25 years, there is an increasing tendency to seek the causes of such a change locally. Thus the old theories of invasion and colonization, are being thrown overboard. The latest in this line of thinking is an article by Adams who examines these theories with particular reference to Egyptian history and shows quite convincingly how the old view of Elliot Smith about the Egyptian people were based on very inadequate or ill-digested evidence (Adams 1968). The beginning was made by Professor Clark (1966) with the re-examination of the evidence regarding invasions in British prehistory. Behind all these attempts to seek local causes for an independent / indigenous development, there may be some lurking desire to prove that the country/region one is discussing was not so dependent after all. And such a feeling may be directly traced to the new sense/pride of nationalism generated after the Second World War.

Different Centers

But there is little doubt that many of the former views about the birth of civilization in the Near East/Western Asia, such as domestication of plants and animals and the beginning of arts and crafts as well as the early views on astronomy were based on very inadequate evidence, of the basic data, as well as the absence of data from Europe, Africa, South-east Asia, America and Australia. Besides a frank appraisal of the old views and evidence, there is also the fact that regions hitherto regarded as backward have produced startling evidence, not only for the origin of rice cultivation (Gorman 1970), but much earlier knowledge of the use of bronze, the use of polished stone tools and cremation (Bowler et al. 1970 : 58). Hence scholars like Higgs and Jarman have challenged the views of scholars Gordon Childe and Braidwood about the life of Early Man, and about the domestication of plants and animals. In

brief, about the birth of civilization itself (Higgs and Jarman 1969).

Not only in his struggle for existence man had taken strides earlier elsewhere than in West Asia, but it is now claimed after much discussion between archaeologists and astronomers that monuments like the Stonehenge in Britain were built as early as 2000 B. C. for the observation of the sun and the moon (Hoyle, *et al.* 1967 : 91-98.)

Even before this revolutionary interpretation of Stonehenge was put forward by Hawkins, Alexander Thom, who was a former Professor of Engineering Science at Oxford has very carefully and scientifically examined a large number of Megalithic monuments stone circles, standing stones-flattened circles, egg-shaped rings, ellipses, compound circles, and alignments-in Britain.

This study has shown that the plans of non-circularings which is almost exclusively a British phenomenon—is deliberate. From this two conculusions are drawn. First, that the builders of the large stone monuments who until now, are regarded as intellectually and materially, "nastic, brutish, and short' possessed a knowledge of geometry and mensuration, of course, emperically, observational astronomy and the calendar. And secondly, this was probably an indigenous tradition and not derived from the literate civilizations of the Ancient East, or Western Asia. (Atkinson 1968 : 77-78)

Later Alexander Thom published another book *Megalithic lunar observations*, (1971). This was reviewed by David Kendall, Professor of Statistics, in the University of Cambridge (*Antiquity* 1971 : 310-15) and Mr. Douglas Hegge of the Institute of Theoretical Astronomy in the same University (*Antiquity* 1972 : 43-48). Both these scholars say that the evidence that the megalithic man observed the moon is so strong that it may be accepted without hesitation. (*Ibid.* 48).

This and the conclusion it points to us is tremendous. We have a large number of such megalithic mounments-not only in Andhra, Karnataka, Kerala and Tamil Nadu, but

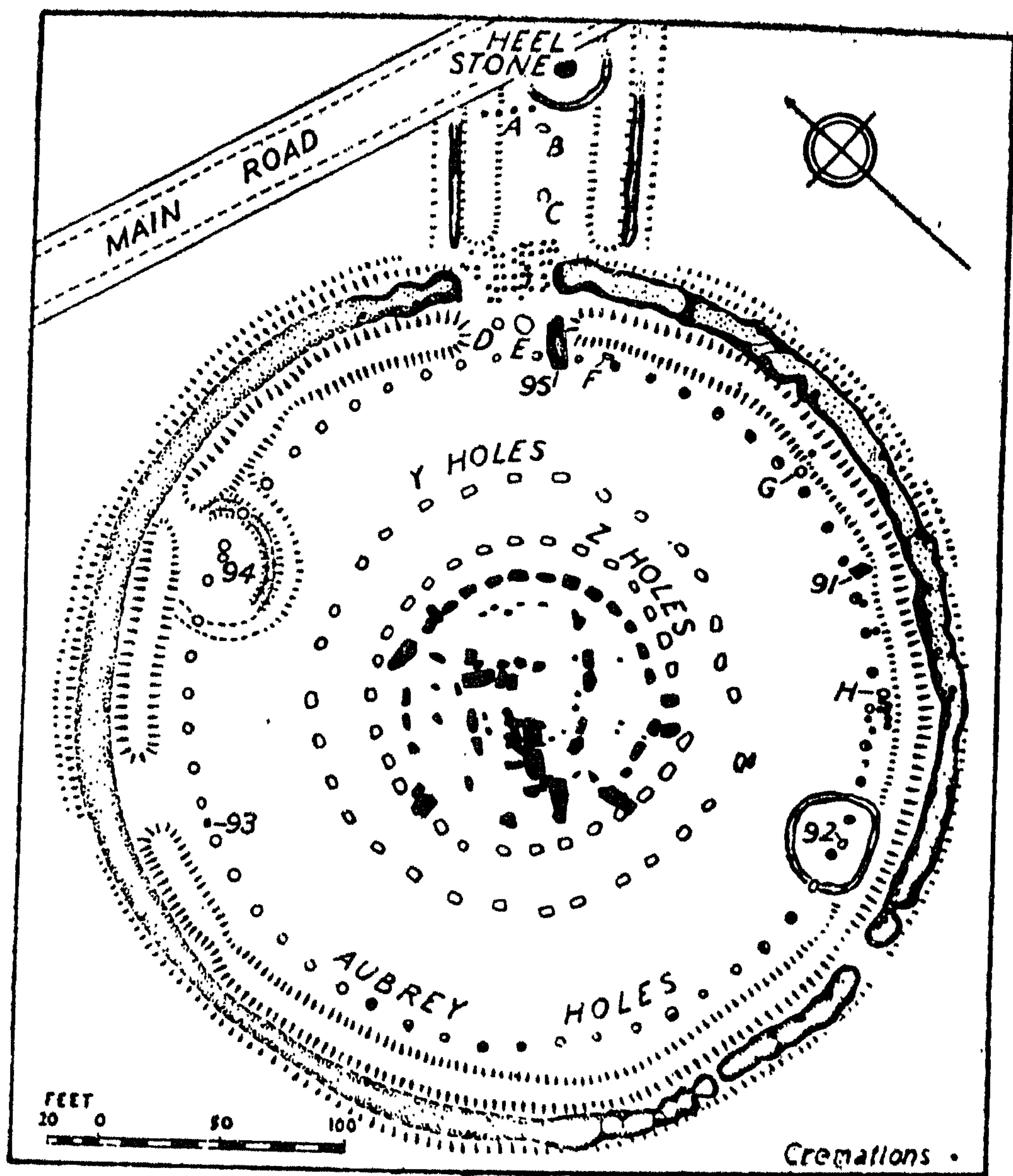


Fig. 1—Plan of Stonehenge

even in Maharashtra—both Eastern and Western. These have been studied, so far only archaeologically but not astronomically. Still more startling is another discovery. It is now held that as early as 20,000 B. C. man had begun to record symbolic notation on the bones of animals he hunted and slaughtered for food (Marshack 1972 : 817-28; for latest discussion see *Current Anthropology* 1974 : 327-331). These examples should suffice to show what tremendous progress is being made in archaeology in our knowledge of the past by the application of science and technology as well as fresh thinking.

But the fresh thinking has not stopped here. It desires—nay claims—that archaeology can become an exact science, it reveals (or should reveal) everything of the past, and also have a logical philosophy so that general laws can be deduced. It is here that the new archaeologists have a head- on collision with the old. Because not only these views go against the very definition of archaeology, but because these views are often expressed in a language or jargon which is difficult even for an Englishman to understand. Hence after considerable discussion Glyn Daniel decided to stop further publication of such views (*Antiquity* 1973 : 95; see appendix I). Nevertheless, it is desirable that students and all those interested in the future development of archaeology should have some idea of this interesting controversy, and above all try to obtain whatever benefits one can obtain by adopting these new techniques and goals.

In the first place, all this discussion centres round what we in India regard as prehistoric archaeology; the various monuments, sites and cities with remains of forts, and such other buildings as caves, temples, mosques and minarets lie outside the sphere of this restricted definition of archaeology. The reasons for this have been given above. Briefly, whether regarded as archaeology or not, there is not much scope for the differences in interpretation about the function of a temple or a mosque, whereas its history and technology may be

gathered from written accounts and by a study of the monuments itself. Things are different with regard to a prehistoric object or site. Next to nothing is often known about it, hence the need for a precise and exhaustive interpretation.

Binford School

Among the various scholars who have written on this subject, Binford is the most prominent. It was he who after the Second World War took to archaeology and studied under such famous archaeologists as Professors R. J. Braidwood, John Bennett and James Giffin. He was not satisfied with their methods and interpretations, and began to think and work independently. Most of his early work on archaeology is connected with late American prehistory. (Binford 1972 : 36). He had also an opportunity to organize a symposium on the problems he was thinking of in 1963 which were beginning to become fashionable at this time. Binford thus became an *avant-garde* (spokesman or leader) for this group of Americans. Hence it is called the Binford School and the subject "New" or "Scientific" Archaeology. (Binford and Binford 1970).

Philosophical and Analytical Archaeology

The subject has been raised to a philosophical level by young scholars like Patty Jo Watson, Steven A. LeBlanc and Charles L. Redman (Watson 1971). In England, David L. Clarke came out with a book called *Analytical Archaeology* (1968). The latest position is presented by Redman (*Research and Theory in Current Archaeology*). Thus I may say that I am fairly well acquainted with the new trends, particularly conceptual, in American and European Archaeology. And it is with a desire not only to stimulate fresh thinking, but make a practical use of it in India that the present effort is made.

David Clarke regards "archaeology as it is known and practised hitherto as an undisciplined empirical discipline" (Preface xiii). He again says, "It lacks a scheme of systematic and ordered study based upon declared and clearly defined

models and rules of procedure." There is the absence of central theory capable of synthesizing the general regularities within its data in such a way that the unique residuals distinguishing each particular case might be quickly isolated and easily assessed (*Ibid.*).

He further says that, "archaeologist is studying concealed and obscure facets of hominid behaviour through the peculiar medium of the fossilized and concealed results of this behaviour, imprisoned in the attributes of ancient artifacts." "It is a discipline in its own right because it alone provides the conceptual apparatus for analysing this peculiar data; a different discipline and different conceptual apparatus from that required for the study of history in its limited sense" (*Ibid.* : 16). But so far, archaeology has remained an intuitive skill, an inexplicit manipulative dexterity learned by rote (*Ibid.* : XIII). In such a narrative of narrative history and paricularist analysis, the central theory is neglected (*Ibid.* : XV). This being the position of the "orthodox archaeology", Clarke thinks that archaeological studies need an all sided approach, a socio-cultural environment (*Ibid.* : XIV).

He therefore advocates the use or application of analytical archaeology. This deals principally with analysis of the data and the sphere of synthesis and theory, but leaves out data collection and excavation.

Of course, response to these new ideas is not universal. Some regard them as historical development—historigraphy, others scorn the whole, while a third group accepts it partially.

The aim of analytical archaeology is to draw attention from specific archaeological areas and periods to the general theory analysing modern archaeology; to refocus our attention on the inconsistancy and inadequacy of the existing archaeological theory, and integrate powerful new methods into one analytical theory.

There are three specific, inter-connected spheres of activity.

1. Data recovery, for example by excavation

2. Systematic description—taxonomy and classification
3. Integrating, synthesizing study generating models, hypothesis and theories (*Ibid.* : 14).

Clarke states his case again and again at the same time pointing out the weaknesses or defects of the present day archaeology. The present day archaeology consists of "mere" or "more" facts, but there is very little more information. He explains the nature of archaeological facts as entities having defined attributes; these specific contextual or both entities and others of similar class of inference. These may be artifacts, assemblages, cultures, sites—all regarded as most useful, most frequent, but also most dangerous. The reason is that these are "preconceived facts" and not necessarily possess or carry with them all the aspects. But these changes in their value according to the light in which we see them change.

Thus "new" archaeology should first collect data, by excavation or otherwise, and then by analytical and statistical taxonomy idealized models or hypotheses should be made about the data received and retransmit these models to the experimental aspects for further testing and modification. By this continuous feedback cycle of observation, hypothesis experiment and idealized model, the model and the hypothesis gradually become accurate, adapted to the pattern of observed data. Gradually the hypothesis may be elevated to theories and ultimately the theories elevated to synthesizing principles if the results warrant these steps. Thus analytical archaeology is primarily a syntactical approach to synthesis and central theory, a changing corpus of central theory, a changing corpus of conceptual frameworks, which emphasize that no archaeological study can be better than the ideological assumptions which underline the development of its argument (*Ibid.* : XV).

Models

Clarke then defines the nature of these models. These are of three kinds.

1. Iconic models

2. Analogue models
3. Symbolic models

Iconic models

Distribution maps, simple histograms fall under the first category. Their usefulness is limited to concise documentation and may be used for the purpose of generalization.

Analogue models

These represent observed attributes whose consequences are congruent to those of observed attributes. These are *currently common and dangerous.* These provide prehistorians with different conceptual framework over what they stretch the same facts but with differing results which can be compared and contrasted for their accuracy or inaccuracy in the light of accumulated evidence. (*Ibid.* : 39).

Symbolic models

"These are models in which the observed attributes are represented by symbols which unlike iconic symbols are integrated in a specific calculus". (*Ibid.* : 33). The last is the hightest or the best kind of models. Hence it is said that "the powers of numerical taxonomy promise faint hope that one day even archaeologists might be able to express their mind models in efficient abstract terms of high predictive values". (*Ibid.* : 39).

From Clarke's point of view "The three age system" alone represents the development of the essential base of the modern analytical archaeology". Very little of fundamental importance has since then been postulated. Later developments or contributions in archaeological theory are a matter of degree alone and not so fundamental (*Ibid.* : 11).

An interesting discussion has followed on these "models." Renfrew tells us that the term "model" was first used by Professor Piggott when he referred to "models of the past" (*Approach to Archaeology* 1959), the different ways in which

prehistorians picture the past, and the resulting differences in the theories which they construct.

A model has two useful meanings :

1. It can refer to the underlying logical framework within which the archaeologists think, called "unconscious models in social anthropology."
2. The second and most restricted meaning now widely used in sciences, refers to a mechanism or a pattern to which mathematical or quasi-mathematical formulations can be applied.

For instance, if the supply of raw material diminishes rapidly as the distance from its source increases, it may be possible to find a mathematical formula approximating to this and then to suggest how such an effect could occur in practice.

Bruce G. Trigger would further like to distinguish between *processual models* and *procedural models.* The first are the models about cultural processes and human behaviour that the archaeologist uses to interpret his data. "These models concern matters such as the relative importance of migration, independent development and diffusion as sources of cultural change."

"Procedural models are assumptions about the relative importance of various classes of data for the reconstruction of prehistory and the manner in which these classes of data can be articulated with one another." For instance, the importance to be given to linguistics, oral traditions, ethnology, etc., besides purely archaeological evidence (*Antiquity* 1969 : 60). Renfrew, however, does not see the necessity of making this distinction between the models. (*Ibid.* : 61)

Aims and Methods of New Archaeology

The aims and methods of New Archaeology have been excellently set forth by Binford (1970 : 5-27), who incidentally if not the first, is the most vocal and forthright critic of the old traditional aim, and methods. Binford, while setting forth the views of the archaeologists, has usefully summarized the

development in ideas and methods since the early part of this century. This itself shows that some (a few) scholars both in Britain and America were dissatisfied with the prevalent aims and methods, but as Binford says, both Piggott and Willey were pessimistic about achieving more than what was being achieved by various workers in the field, because of the fragmentary nature of the archaeological record. Binford then very briefly analyzes the traditional approaches. He then concludes, "These procedures, however, do not help to achieve the stated aims of archaeology, viz. (i) the reconstruction of culture history, (ii) reconstruction of the lifeways of extinct peoples. An accurate and meaningful history is more than a generalized narrative of the changes in composition of the archaeological record through time; it is also more than a reconstruction from that record using interpretative principles (such as the degree of genealogical affinity between two cultural units)". Further, if we hope to achieve the aim of reconstructing culture history, we must develop means for using archaeological remains as a record of the past and as a source of data for testing propositions which we set forth regarding past events, rather than as a record we can read accordingly as a set of *a priori* rules or interpretative principles whose application allow the skilled interpreter to "reconstruct" the past. We know much too little about both archaeological data and process of cultural development to make "reading the archaeological record" anything but a shallow and suspicious pastime. (*Ibid.* : 12)

To achieve the second goal, viz., reconstruction of past lifeways, archaeologists have been increasingly studying the living peoples. But here says Binford, "the major controversy has concerned the appropriateness of a given ethnographically known group or set of conditions as a model for the lifeways of the groups under archaeological study." (*Ibid.* : 12). He no doubt welcomes the refinements in data collection, and increased ethnographic knowledge, "still these cannot by themsevles increase our knowledge of the past." Then what

shall we do ? Binford proceeds, "Facts do not speak for themselves, and even if we had complete living floors from the beginning of the Pleistocene through the rise of urban centres, such data would tell us nothing about cultural or past lifeways, unless we asked the appropriate questions." (*Ibid.* : 13).

"The archaeologist should not only generate inductive inferences, but he should develop independent means of testing (his) propositions about the past". (*Ibid.* : 17) "...Our knowledge of the past is more than a projection of our ethnographic understanding. *The accuracy of our knowledge of the past can be measured*; it is this assertion which most sharply differentiates *the new perspective from more traditional approaches.*" (Italics mine). There should be a conscious shift to deductive philosophy, with the attendant emphasis on verification of propositions through hypothesis testing. Binford and his followers do not believe that archaeologists must limit their knowledge to the features of material culture; "in fact, there is no dichotomy between material and nonmaterial aspects of culture itself...." It is virtually impossible to imagine that any given cultural item functioned in a socio-cultural system independently of the operation of "nonmaterial" variables. Every item has its history within a socio-cultural system...." (*Ibid.* : 21).

"The crucial question for archaeology is the relationship of our observations to the operation of past cultural systems.... Do our stone tool typologies measure function or style, or do the attributes which define types involve two or more variables?" (*Ibid.* : 25).

"The elaboration of theory and method which characterizes much of the recent work in archaeology consists minimally of two elements : first, the active search for understanding variability in the archaeological record—all of the variability and not just that judged *a priori* to be significant; second, an attempt to explain variability scientifically, rather than by conjecture or by "hunch."...Many kinds of variation will be shown to be the result of the normal functioning of internally differentiated cultural systems; others may

document evolutionary changes within cultural systems. Still other kinds of variation may reflect changes in content with an essentially stable cultural system. In our search for explanations of differences and similarities in the archaeological record our ultimate goal is the formation of laws of cultural dynamics." (*Ibid.* : 27).

Variability of human behaviour

The new archaeologists argue that in the past scholars—fieldworkers as well as synthesizers—have taken implicitly for granted that the human behaviour was more or less uniform. Hence whenever they came across a different type of tool or a piece of sculpure or architecture or any such material remains, it was attributed to a different people, a new arrival or a new influence. They had no idea of the complexities of human nature. This is much more difficult to understand than animal behaviour. It is argued that "there is a great deal of variation in human behaviour, probably more than in the behaviour of any other animal....This is partly because human beings, like the mammals in general, are the result of a long period of positive selection for behavioural adaptability rather than for behavioral specialization. This adaptability has also been influenced by the cushioning effect of culture" (Watson *et al.* 1971 : 161).

Thus unilinear or vertical development of human culture, development as postulated by archaeologists in the past owing to the changes in pottery or stone tools with or without corresponding change in stratigraphy as an indicator of cultural change has been questioned. There is probably some justification in regions/countries where there are marked environmental changes, e. g., Western Europe, or where owing to some other reasons people have occupied different "niches" in a specified region, and there is a chance of "differential cultural" development as witnessed in or documented by the archaeological data. But as pointed out elsewhere in countries like India where there are no contrasting seasons, and where owing to

this or other reasons human nature tends to become conservative and tradition-bound, it is not easy to postulate changes in the human behaviour from the nature of the finds.

The best way to understand what Binford and his colleagues advocate so vehemently is to study a little critically their own contributions in this book, as well as others such as *Explanations in Archaeology*, and *Current Research in Archaeology*, copies of both of which were sent to me kindly by Watson.

The *New Perspectives in Archaeology* contains four parts. The first two parts contain five essays each, the third one has six, and these are followed by Part IV on Discussion. These 16 essays deal primarily with the question of variability and change. Traditionally archaeologists are accustomed to explain a change in an archaeological record, as due to arrival of a new set of people (either as refugees or conquerors), or as due to diffusion (which might be direct and hence quick or slow).

The new archaeologists question these *a priori* set of inferences and emphasize the need to look for the explanation of this change within the data itself. Thus Sally Binford has tried to attribute changes in the Mousterian artifacts from the sites in Near East (Palestine) previously excavated by Dorothy Garrod as follows. In a single culture site, Variability can be profitably examined within a functional frame of reference. Variability between assemblages at the same site may well represent "activity variants," not necessarily "culture change", or occupations by bearers of different "traditions". These subtle distinctions between the various phenomena we might be able to distinguish by various (new) analytical techniques.

When the sites are different and there is also difference or variability in the archaeological record, these might represent differences in settlement type, and not necessarily indicate a change or difference in culture.

The problem was to explain the occurrence of a certain tool type called 'Emireh Point'. This is always made on a Levallois Point (Garrod 1951; *JRAI* 81 : 120-30; Binford in

Binford and Binford 1970 : 54). This tool type is regarded as a hallmark of a culture between the Mousterian and the Upper Palaeolithic in certain sites of Palestine. Garrod regarded it as a transitional industry between the Mousterian and the Upper Palaeolithic. Sally Binford, after a fairly detailed discussion of the nature of the deposits where it occurs, feels that since there is no clear stratigraphical difference between the two cultures, the technological change—the consistent manufacture of implements by punch-blade technique, and a marked increase in the frequencies of composite tools—might be explained or hypothesized as due to new kinds of relationships between man and his environment both physical and social. "The emergence of new biological and cultural forms is the end product of adaptive change, and it is in the nature of the basic adaptive changes that we will begin to understand why and how new forms appeared." (*Ibid.* : 56).

And it is claimed "that if new methods of data collection and analysis (such as factor analysis) are employed, we can certainly learn much more about the internal divisions of local groups and between group relations and how these might have changed throughout the Middle Palaeolithic." (*Ibid*).

It is further argued that a shift from generalized to specific hunting is suggested from the predominance of the *Bos* fragments at Skhul, one of the Mount Carmel caves. From this one might infer about changes in man's relation to his physical environment, but from this total dependence upon *Bos* can we say anything about the type of stone tools, granted that we must distinguish between stylistic changes and functional changes in lithic assemblages ?

In the end, it is said that "until the Mousterian and the early Upper Palaeolithic are both analyzed in terms of units which are relatable to human activities (that is tool kits) and their spatial distribution (settlement types and patterns) we have no means for accurately measuring the degree of difference between the Mousterian and the Upper Palaeolithic cultural

systems." (*Ibid*).

All these are good or pious expectations and some inferences. They certainly give us food for thought, but whether such differences do really exist or take place are also questions worth asking. Is not the emphasis on "change" itself not a new feature and particularly of the American life, where because of various reasons, abundance in everything—food, accommodation in space and money—a change has become a part of life, whereas in a country like India and many countries of Asia, such changes are indeed rare.

As for the actual significance or the function of tools, Sally Binford has made some intelligent guesses on the strength of factor analysis of the 63 Mousterian artifact types (Bordes 1953a; Binford in Binford and Binford 1970 : 51). Thus in Group I (called Factor I) tools consisting of borers, becs, endscrapers, atypical burins, and naturally backed knives are supposed to be useful as secondary tools and perhaps for hide finishing, and indicative of a base-camp type of settlement. Group II (Factor II) consists of all kinds of points and sidescrapers and is believed to be a hunting and butchering tool kit.

In Group III (Factor III) we have backed knives, naturally backed knives, end-notched pieces, typical and atypical Levallois flakes, and unretouched blades. Except the end-notched pieces, all are fine cutting implements, but because of their association with fire layers the speculation is that these tools might have been used in the preparation of food.

In Group IV there are (briefly) the denticulate scrapers with abrupt retouch, raclettes, and truncated flakes. Bordes' suggestion that the deniculate Mousterian is associated with the processing of plant materials is cited. In Factor V's chief features—"tightly associated element" are : elongated Mousterian points, discs, scrapers on the ventral surface, typical burins and unretouched blades. From this group, only one kind of point and one kind of side-scraper are chosen as a diagnostic cluster, and it is

suggested that this group represents a more specialized hunting and butchering factor than was seen in Factor II.

For all these functional interpretations we have no other contemporary archaeological evidence, nor is there any reliable ethnographical support, as no aboriginal tribe either in Africa, Australia or anywhere where pockets of such people survive, use these types of artifacts. Our interpretations are based partly on what we do today with steel implements. This is at the back of the suggestion that backed knives, Levallois flakes, etc., of Factor III are fine-cutting implements, and might have been used in the "preparation of the food." Now this expression is vague and enigmatic. It reminds us of a similar expression used for explaining the function of the cleaver. First this was supposed to be for cutting trees. But when very few cleavers were found with battered edges, it was suggested that the cleavers were primarily used as meat mattocks. Professor Desmond Clark himself gave up this interpretation and thought that the cleavers were used for "food preparation". (*Prehistory of Africa* 1970 : 100). If we analyse this expression it might mean that this large, heavy tool was used for cutting into smaller pieces fruits, grubers, vegetables, as well as lumps of flesh removed from animal bodies.

This process was probably turned into a fine art during the Upper Palaeolithic by the employment of Levallois flakes and backed knives. The dish called "minced meat" might have been first made at this time ! And the Mousterian "denticulate" might have been used for preparing from vegetables a dish like shredded carrot, or as has been supposed on the analogy of the use of denticulate steel knives by Red Indians that denticulate blades were used by the Mousterian and later Stone Age people in France for shredding meat, and then drying it on a line !! A thin denticulate blade might be used as a saw, particularly on bone and ivory (though it was used in the Mousterian for such a purpose, we have no means of ascertaining). These are of course fineries. Whether the Early Man and his successors

actually indulged in these luxuries is a different question.[1]

The fact is that inspite of analytical deductions made with the help of new techniques we cannot help speculating about the actual function of stone tools. This being the position about the interpretation of the function of stone tools, the further interpretation about the probable social groups and settlement pattern will still remain problematical. We might ask one more question, "Do various social groups, however much differentiated, really use the various tools so differently ?" My experience of conditions in India tells me that this is rarely so. Hence, though we might distinguish between various tool types according to factor analysis and other techniques, still these clusters might tell us little about the then prevailing social groups.

James Deetz (Binford and Binford 1970 : 41–48) in his paper on "the Inference of Residence and Descent Rules from Archaeological Data" tries to make explicit four socio-cultural inferences from archaeological data :

The first level concerns individual behaviour as reflected at the attribute level. For example the hand made pottery in socalled prehistoric site in America has cord-impressed decoration and lip thickening on a rim sherd. Such a grouping of attributes might be seen in the work of an individual and "is archaeology's only case of perfect association."

The second level of patterning is that which results from actions of members of various minimal groups of interacting individuals, lineages, families hunting groups, males or females, though here Deetz warns us that artifact groupings are vulnerable to mixing and imperfect recovery due to the field and in laboratory procedural error.

---

1. After writing this I read in Pfeiffer's *Emergence of Man* that in the cave of *Combe Grenable,* in the Dordogne, post holes were found, which it is thought might be for erecting wooden posts on which such lines of minced meat were hung, dried and then later used for food.

He cites examples again of recent studies of some 19th century sites, and notes in particular how artifacts indicating male activities had disappeared or were rare ; those suggestive of female activities such as baskets, mortars, pestles had survived. Such objects have great significance in America where Prehistory's lowest limit is 18th-19th century A.D. ! In India such objects are merely noted, but no particular inferences are made, because we take for granted that these objects are indicative of female activities in the past and even at present (in many regions). But a good example of a family activity in the Chalcolithic times is the manufacture of blades from chert and chalcedony nodules. As I have been observing since 1959, this must have been a cottage industry practised all over India. Only we cannot go further and say that it was the work of the males or females only.

The third level of patterning is that of the community. This might be inferred, says Deetz, from the spatial arrangement of houses and the form of their architecture. Such an inferential knowledge might be possible again in America, where the entire habitations of Navajos, and other "prehistoric" Americans can be laid bare with comparative ease.

In India we could have done this at Mohenjodaro, Harappa, Lothal or Kalibangan, everywhere or at all sites where excavations were large enough. There was some possibility at Navdatoli. Inamgaon however supplies us the best illustration. Here the houses in the last phase (Period III) are found closely set, and all are round with varying sizes, while in the earlier phase (Period II) the house plans are square or rectangular, and some space is left between two houses. Further, in February–March 1975, we dug towards the centre of the mound in the expectation of finding the residence of the chief or headman of the village. And luckily we uncovered the ground plan of a five-room house, 75 feet × 30 feet with a unique pot burial—an adult buried cross-legged in a sitting posture in a four-legged clay jar.

From this one may pass on to the fourth level where the

entire community is considered as coordinate unit. This level archaeologically would be a study of settlement pattern. And here he adds a useful and a very necessary qualification. The very term pattern denotes repetition. Unless a number of examples of similarly patterned phenomena can be produced, sound inferences cannot be drawn.

After briefly illustrating these four patterns, Deetz discusses how residence and descent rules in a family might be discerned from the archaeological record. But no convincing case has been made out, because as he himself says, "one cannot say with certainty that residence was the sole causal factor in attribute clustering," nor to what degree clustering must occur to demonstrate matrilocality conclusively." And so he has to admit that "Inference of descent from archaeological data, at least in terms of patterning, is a much more difficult and complex problem." (Deetz in Binford and Binford 1970 : 47)

James R. Sackett makes a critical appraisal of the "Method and Theory of Upper Palaeolithic Archaeology in Southwestern France" (in Binford and Binford 1970 : 61-83), and suggests new approaches (or goals) and methods and techniques for reaching these. He is no doubt right in saying that until recently (1950), French archaeologists were mainly actuated to establish an evolutionary sequence of stone tools, and not enough attention was paid to contextual records of tools, and macro-faunal remains that were considered to be primary chronological indicators. Little palaeo-environmental data—records of tools, animal fossils, not only major but micro, and the soil layers in which these were found—were systematically collected (*Ibid.* 67). Secondly, the lithic tools were studied subjectively. Sackett would replace these intuitive type designs by "*attribute-cluster analysis* which isolates typological categories by means of an explicit quantitative analysis of the pattern in which formal attributes segregate non-randomly (that is cluster) among artifact samples." (*Ibid.* : 73). A type defined by this approach, it is claimed, is in reality no more

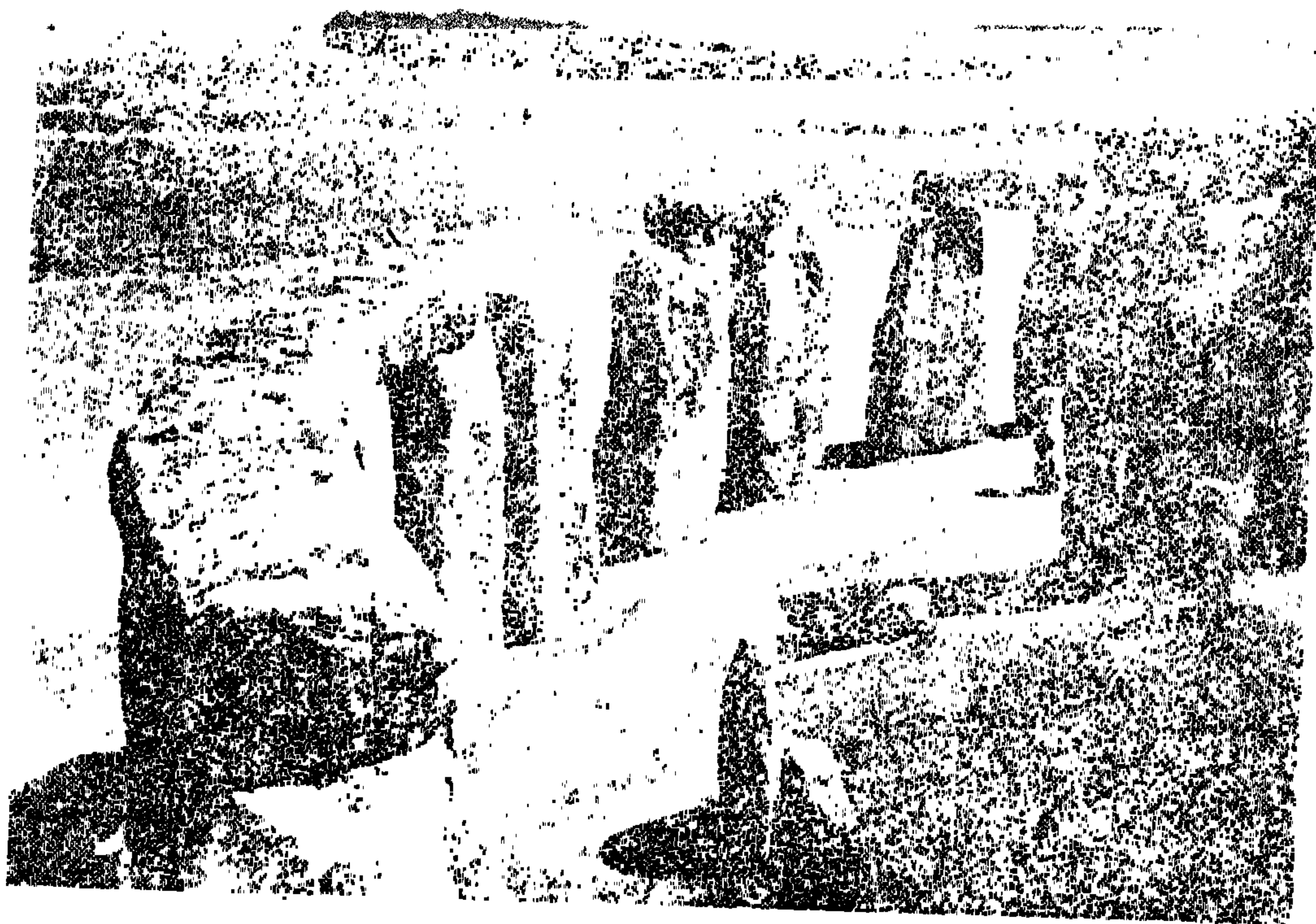


Plate 1. The curvature of the lintels over the entrance

[p. 15

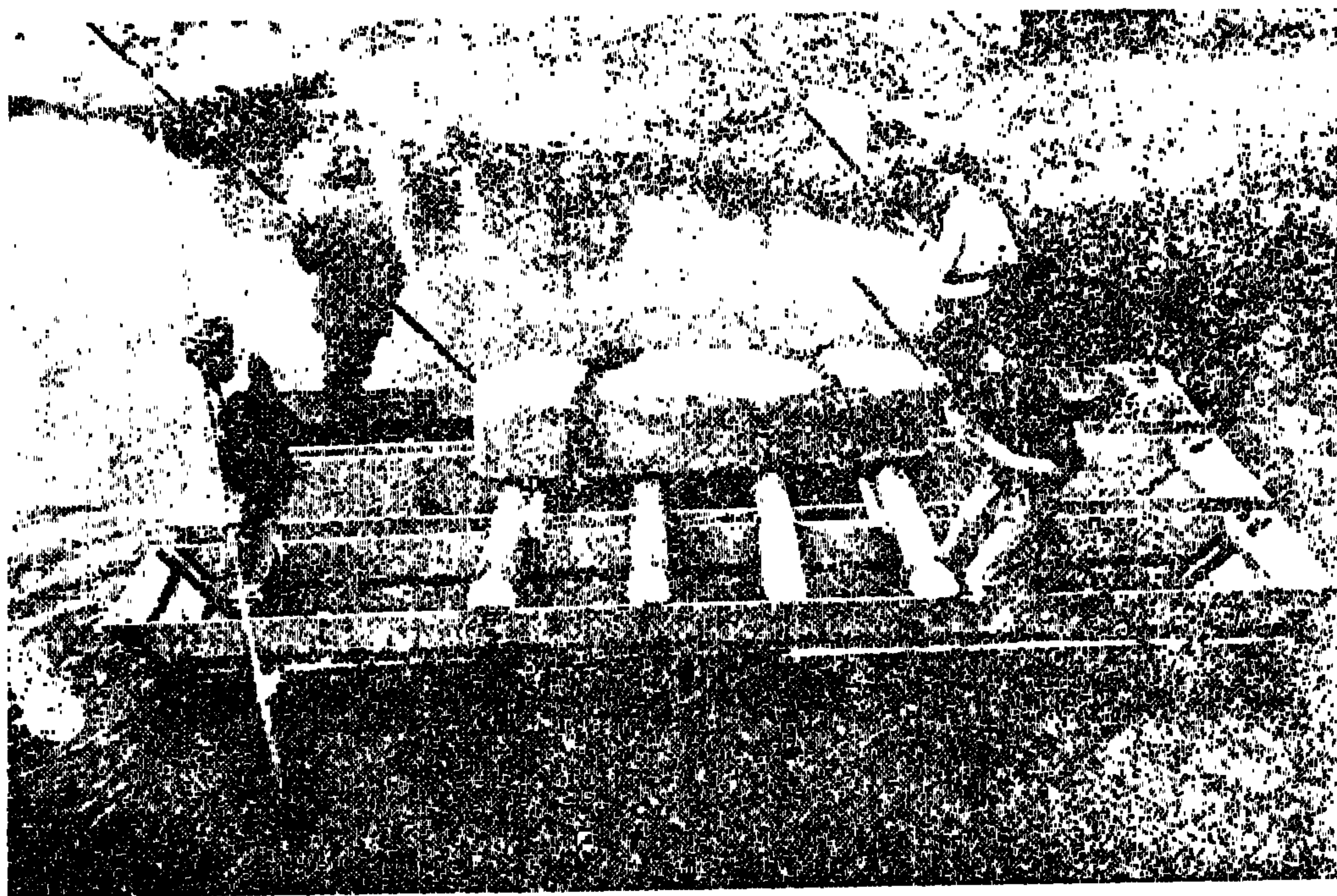


Plate 2. The blue star on a composite boat (to ascertain how such large stones were brought from a long distance)

[p. 15

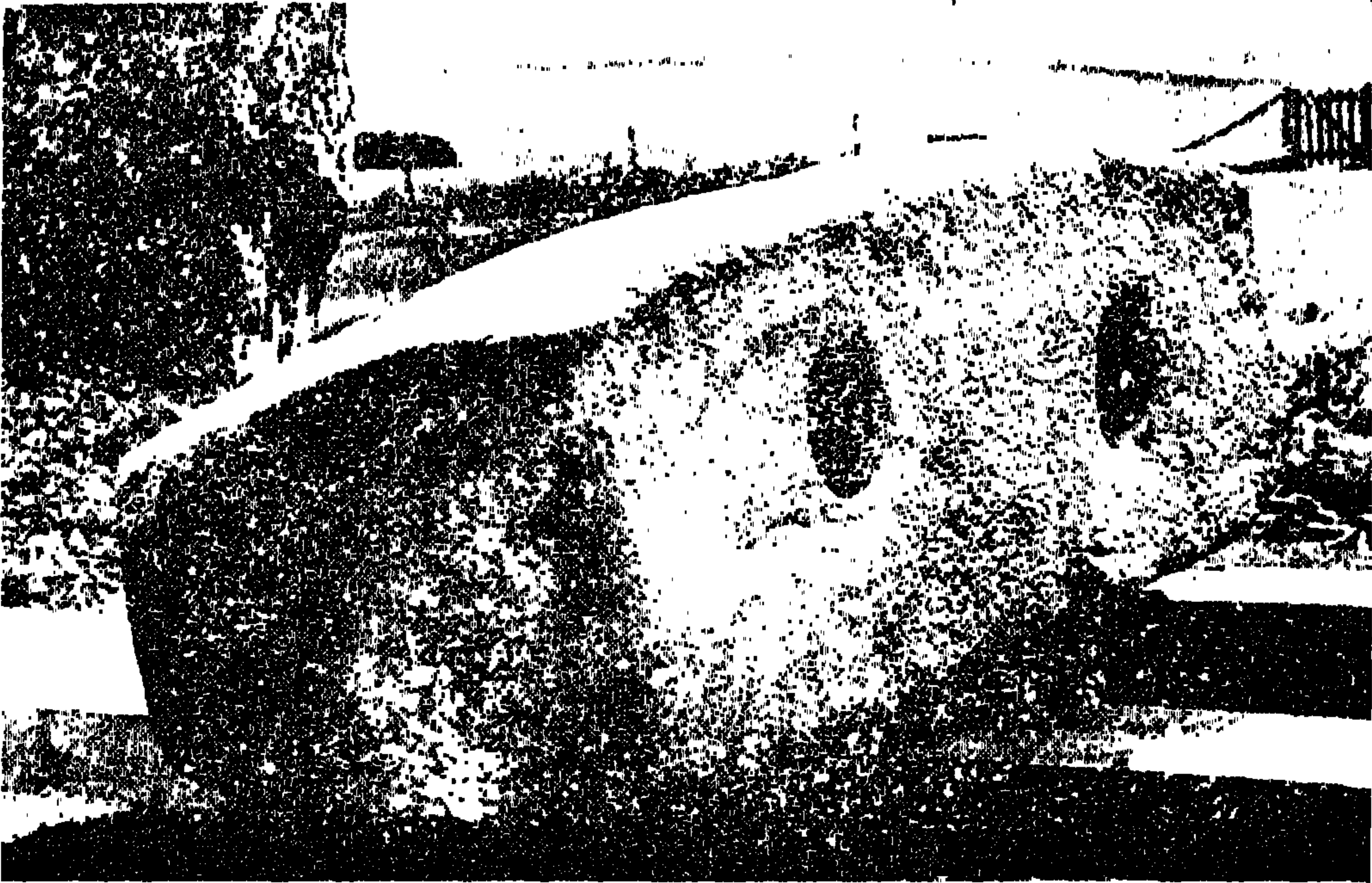


Plate 3. The blue stone lintel 36, showing the seating for upright

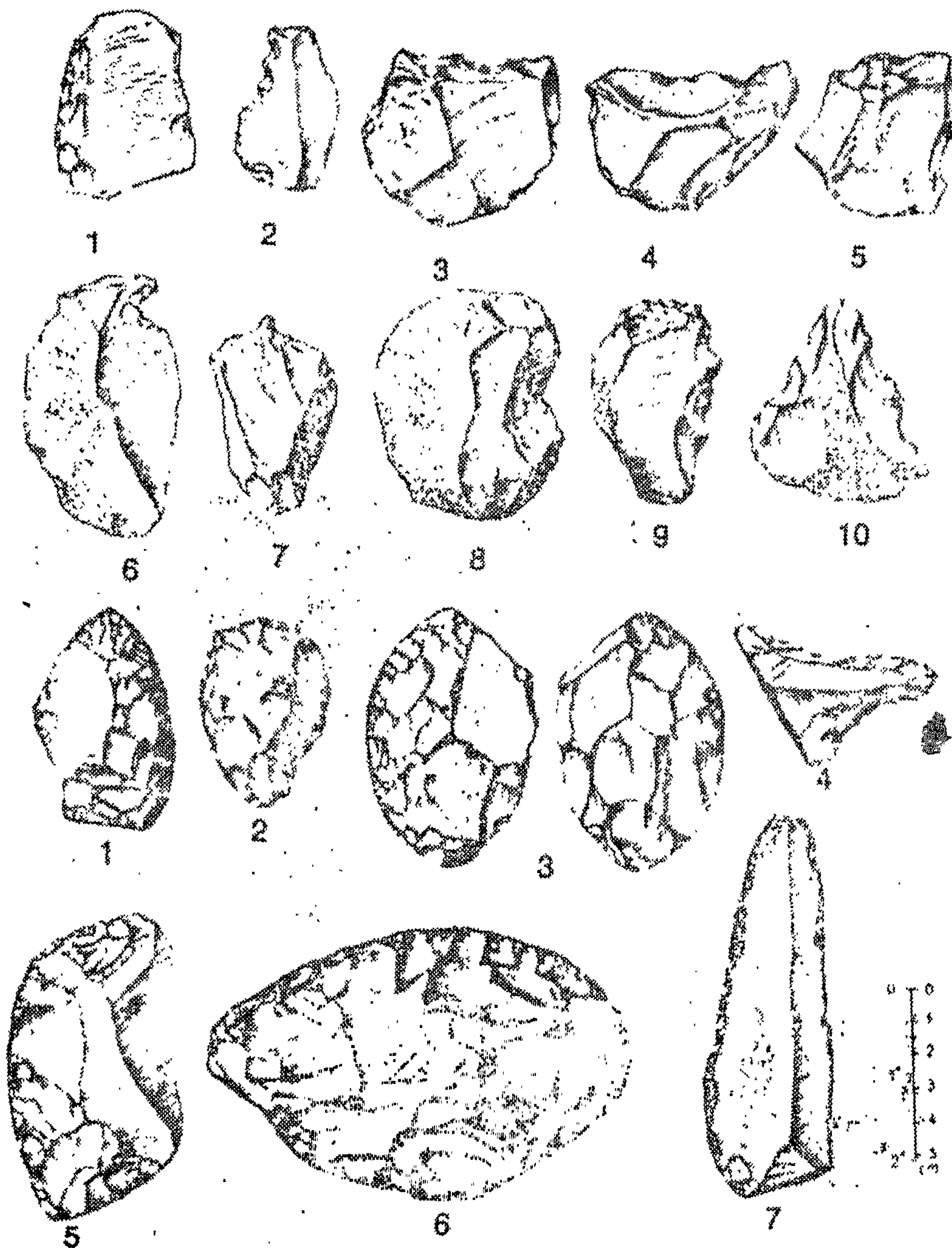


Plate 4. Two Neanderthal tool-kits from Combe Grenale
(Reproduced from Pfeiffer, *The Emergence of Man.* p. 183

[p. 28

Plate 5. Plan of Broken K. Pueblo. Shaded rooms were not excavated. (Reproduced from Binford and Binford 1970, p. 105)

[p. 37

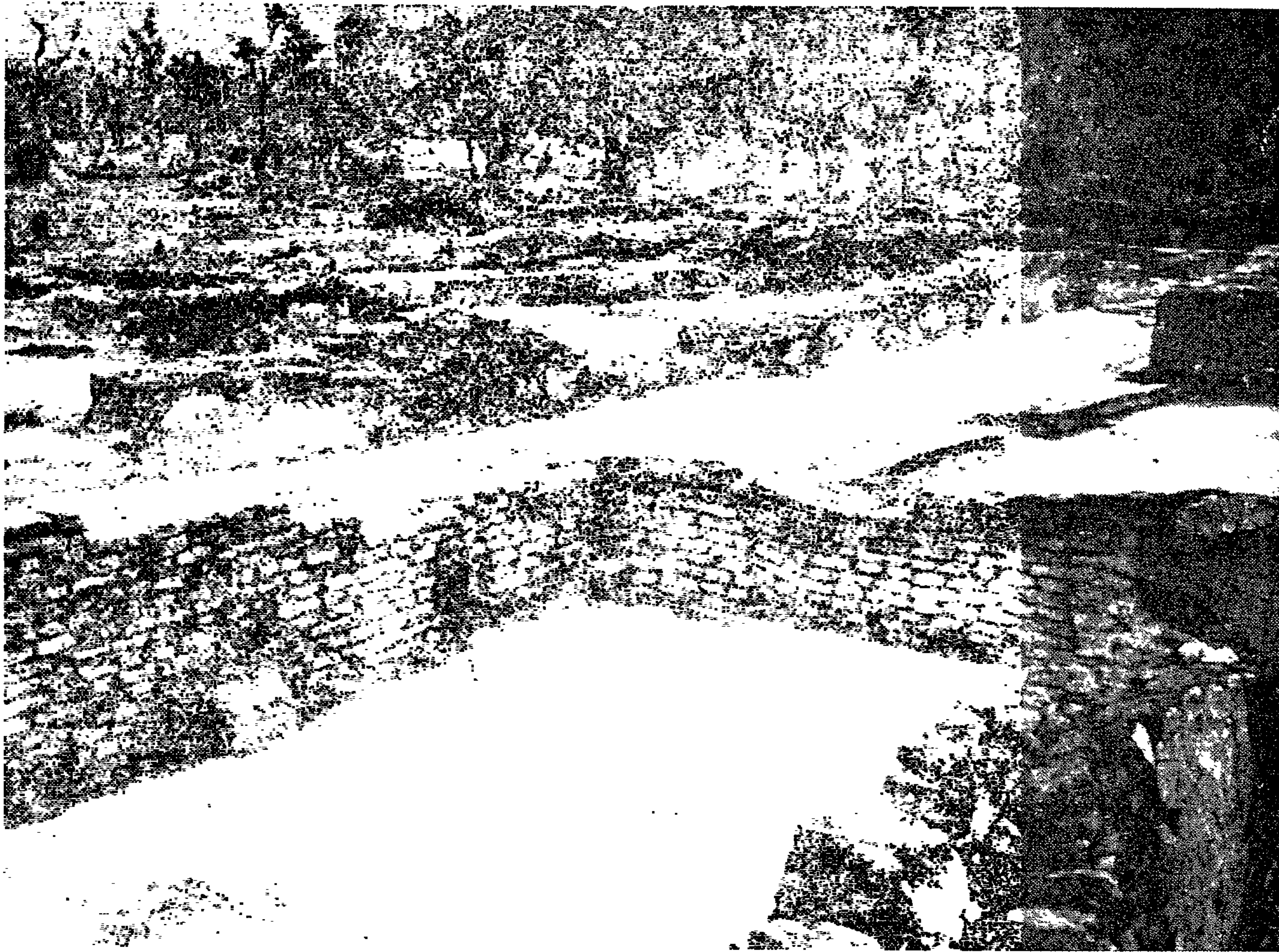


Plate 6. View of the rooms and courtyard of Amir's (Nobleman's) house at Champaner, District Panchamahal, Gujarat

[p. 38

than a particularly discrete variety of cultural activity. Thus the well-known (intuitively or subjectively recognized) Aurignacian end-scraper categories failed largely to equate with the attribute clusters revealed by statistical analysis. Secondly, it should be possible to distinguish between the stylistic and functional variation. Sackett therefore concludes that "there is no single typology inherent in end-scrapers." There would be one typology in end-scrapers based on stylistic consideration to understand space-time systematics (that is chronological evolution or development), while there would be another based on functional considertions and these would help towards the understanding of economic activities among archaeological sites.

Thirdly, the behavior of the lithic arttributes might have been dependent upon the raw material and the stone technology.

In short, instead of the former, single reason (or factor) for a change or difference in tool types from layer to layer or site to site—and this largely attributed to arrival of new people or cultural influences—Sackett would propose several, or atleast three. And if to these three we might add the biological differences, we can imagine how complex the problem of understanding a change in the archaeological record can be ! He also points out the statistical difficulties of isolating meaningful stone tool types and the need to reexamine some of the theoretical assumptions that are currently employed in interpreting the meaning of typologies. He further tells us that a particular type need not be "the material expression of mental models" because it might have been mechanically caused. Likewise, while the attribute-cluster analysis might help isolate culturally meaningful typological patterns, their behavioral significance might still elude us. (*Ibid.* : 74-75)

The function of tools cannot be really understood unless they are found in a known context. How, and where these are found, in what palaeoecological context, is of real importance.

Sackett would also emphasize the need of revising the present taxonomies and procedures that are used in the space-time ordering of archaeological assemblages, particularly in the Upper Palaeolithic where many heterogenous forms seem to occur which suggest the presence of both a distinct type of cultural systems and a considerable amount of micro-evolutionary change within systems. (*Ibid.* : 76).

However, Sackett is conscious of the fact that these techniques would not enable an archaeologist *to find out the social groups, only greater refinement in assemblage ordering than is now generally available will be the net result.* (Italics mine) We might have additional information about cultural continuities and discontinities and demographic patterns of settlement. In the case of the Aurignacian it may be possible to define subregional stylistic zones that correlate highly with river valley systems.

Still none of *these techniques by themselves will inform us about the full range of activities of Upper Palaeolithic societies. For such an intimate knowledge systematic investigation of the spatial distributions of activities both within individual sites and among the site complexes that make up the settlement patterns of cultural systems are necessary.* (Italics mine)

Finally, for an understanding of the Upper Palaeolithic settlement pattern not only caves but open-air sites in the Dordogne have to be excavated. Here the problem is "should we concentrate upon the complete recovery of a relatively few occupation levels in order to refine our techniques for distinguishing the multiple activities that were engaged in by individual residence groups? Or rather ought we to concentrate upon randomly sampling a large number of sites hoping to increase our knowledge of the total variety of activities and demographic patterns that occurred in the Upper Palaeolithic as a whole?" (*Ibid.* : 79) Considering the funds and time that is needed for refined excavation, the development of culturally meaningful sampling techniques is of paramount importance in the development of future research strategy. And in the end, in spite of all this advice, Sackett had to concede that the

present absorption of archaeologists in data collection and descriptive classification is obviously not wrong in itself, as must necessarily precede meaningful cultural analysis. (*Ibid.* : 80).

How to find out the existence and nature of the social units from an archaeological record ? It is argued that these might be inferred (found out) if when an excavation is conducted enough care is taken to recognize, measure, and explain the variability in the form and distribution of a site's cultural remains. It is argued that internal variability is to be expected and that it derives from the differential composition and distribution of the societal segments represented at the site. These segments can be distinguished by their activities and tasks and by their cultural means of relating to one another. (Binford and Binford 1970 : 85).

The isolation and definition of ordered relationships in the archaeological record are necessary for empirical generalizations about the record; these generalizations then can serve as the refinements for our explorations.

"An assumption basic to this process is that the archaeological deposits are undisturbed and have been investigated in a manner which takes the sampling error into account and that order in the record derives from the systematic relationships which characterized life of the unit under study." (*Ibid.*).

Now the assumption and its corollary are most important for any consideration of the application of these new methods and the associated goals to sites in India and Western Asia. How many sites can we find where we can assume that the site in question has been undisturbed by man and nature during the hundreds and sometimes thousands of years that it has been abandoned ? And if we are not sure of its undisturbedness how can we accept the corollary ?

This assumption migth be possible in America where prehistory's one end of the bracket is around 1600 A. D. and where the descendants of these so-called prehistoric inhabitants

still live and hence we can with this ethnographic knowledge make some meaningful inferences.

But it is doubtful, nay hazardous, to extend all these techniques to a site which is several thousand years old, and belongs not to one period, but to several periods. However, for the future guidance of our scholars, a brief summary of the results and methods followed to achieve these results is given.

Longacre (in Binford and Binford 1970 : 89-102) approached the Carter Ranch site in East Central Arizona. Though it is called a study of prehistoric society, the site is hardly thousand years old (c. A. D. 1050-1200), and fortunately it has been well preserved. However with a little but careful excavation of the surperficial debris, the whole layout of the sites with its rooms, and their belongings was found as they were left. This being the case it is not difficult to imagine or infer the function of various rooms; some rooms with soot marks for cooking, some for ceremonial purposes, others with silos or grain bins for storage purposes. Particularly significant from our point of view are the inferences regarding the existence of social groups and inheritance, based on the study of pottery designs.

"At the Carter Ranch Pueblo, 175 design elements and element groups were analyzed in terms of their distribution in rooms, kivas, burials, and trash. The designs clustered in association with two major architectural units at the site. On the assumption that the females were the potters, this patterned distribution argues for post-marital residence in the vicinity of the wife's female relatives with ceramic decoration learned and passed down within the residence unit. Time depth is demonstrated by the association of designs on pottery in the architectural units and associated trash in deposits of over one meter deep."

"The localization of females in architectural units at the site over a period of several generations suggests in turn that non-portable objects such as rooms and access to a specific mortuary area, were inherited within the residence units and

this inheritance was probably in the female line." The corporate nature of the residence unit is also suggested by the maintenance of a kiva and by associated mortuary practices.

Now such inferences were made possible by two factors. First, the assumption about the females as potters. This is probably based on some ethnographical evidence. Secondly, the site was found almost intact, so that one could be sure of the position of various objects, and their functional significance. And with regard to the logical validity of the hypothesios. Professor Morgan has pointed out that the reasoning is defective and no general law can be deduced. (See below pp. 46-48).

James Hill's paper "Broken K Pueblo : Patterns of Form and Function" (in Binford and Binford 1970 : 103-142) deals with a similar site and of the same antiquity. Here a 95-room site was first "scraped with a tractor," then "extensively well-trenched" to reveal major features. It is not clear how the objects (cultural materials) were collected or noted. "These were screened with a 1/4 in. sieve in an attempt to ensure comparability of samples." Materials found directly on the floors of the rooms were kept separate from those in the fill levels, since in isolating the functions of the various rooms the materials lying in the floors are clearly most informative. (*Ibid.* : 106).

Nobody would expect that all the 95 rooms, or the 54 rooms that were actually excavated *would contain identical materials. Hence variability in rooms was expected*; this was *noted* and has helped in inferring about the general and specific function of various rooms. The presence of fire pits and mealing bins in large rooms has led to the inference that cooking and food preparation were practised in these rooms, whereas there were special rooms (called *kivas*) for ceremonial purposes. Further and more detailed variation in the rooms was studied after noting their size, firepits, mealing bins, ventilators, doorways, height of the door sill and masonry style. Later all these details were analyzed with the help of computers, then the final results checked against the ethnographic

data from modern Pueblos.

While one appreciates all these methods and appoaches, a question might well be asked "how far this paper (excavation) serves as an example of a general methodological approach that should prove useful in nearly all archaeological studies, regardless of the kinds of archaeological remains being considered" and again how can one test these inferences when "direct ethnographic evidence is not available" ? (*Ibid.* : 104) Obviously, Hill is unaware of the complexity of the truly prehistoric sites in India, Western Asia and Europe, and the magnitude of the problems in historic sites, say in India. At best, we could follow Hill's example, if not already done, in the excavation of such one period sites as Champaner and Fatehpur Sikri, which are not buried under thick debris, and where it would be possible to test the inferences by reference to modern analogies, and also by a study of the contemporary literature. And even then we should think twice before the surface of these sites is removed with the help of a tractor ! (though I am told that this is quite feasible in American sites).

Further, as I have said elsewhere, even more ancient sites like Taxila, Kausambi, Nalanda which have remains of several periods, but where structures built in stone and brick are fairly well preserved, we could do much more than record merely images, sculptures, coins, and the like. A contextual study of every thing—including animal bones—would really tell us about the life of the inhabitants of the structures excavated, if not the entire site, as each was indeed a city or a town. The current excavations at Vikramasila should really have this objective in view, and so also the excavations at Mathura, Ayodhya and Kausambi whenever these are undertaken.

Can archaeologists infer something about past social organization from the stylistic behaviour of material items ? Yes, according to Robert Whallon Jr. who undertook the investigations of Late Prehistoric Social Organizations in New York State. (Binford and Binford 1970 : 223-244).

The hypothesis that measurable relationships exist between

style and social organization is founded upon two basic assumptions. The first is that style has many aspects and levels of behavior which may be analytically distinguished and measured. The second assumption is that the nature of the diffusion of stylistic ideas and practices, both within and between communities, will be determined by the nature of interaction among artisans. The aspect of style concerned, the rate of diffusion, and the directions and limits of diffusion will be conditioned by the kind, frequency, and channeling of interaction among the producers of the stylistic material. These patterns of interaction either define, or are the result of the social organisation of the community or region. However what style is is not easy to define. "Initial selection of attributes is made largely intuitively though often with consideration of experiences in previous analyses. If these attributes exhibit systematic patterns of behavior which can be related to the influence of social, cultural or individual factors rather than to factors of function or of physical environment both the attributes and their behavior are considered 'stylistic'." (*Ibid.* : 224).

The second basic assumption had proceeded on an earlier assumption by Deetz that if there was a community with a high rate of matrilocal residence and if women made the pottery, then this would be reflected in the archaeological data (even potsherds).

Explaining further it was predicted that "if residence is generally not in the village of the bride, women will move about among several villages, bringing with them all their characteristic techniques and styles of pottery manufacture. Under this hypothesis, the presence in a community of women who have come from different villages at marriage should create a situation of relative heterogeneity of style. Conversely, a high rate of village matrilocality should be characterized by a greater stylistic homogeneity within each village". (*Ibid.* : 229). It is not neccessary to follow further inferences based on ethnographic evidence. Before archaeologists in India can ever think of

such archaeo-ethnographic studies aimed at socio—economic inferences about a site—whether historic or prehistoric—we shall require as a minimum the following :

1. Data from several sites (or villages as in New York State).
2. Some ethnographic knowledge about the communities in the immediate and even distant past.
3. Detailed study of artifacts, pottery, etc. from these sites. These should be all one-period sites, otherwise our inferences at most will be applicable to the latest occupants of the site.

Another difficulty we might face is, as far as pottery is concerned, that with the beginning of historical period, painting on pottery almost suddenly disappears. And thus this diagnostic trait which characterises pre-American cultures is absent in India.

Kent Flannery and Michael Coe (in Binford and Binford 1970: 267-283) attempt an investigation of social and economic systems of Formative Mesoamerica. This study again relies mainly on previous ethnographic data—both literary and actual—particularly Polynesian social organization and its relation to economic systems in Polynesia by Marshall D. Sahlins. Still in the end the authors have to say, "Since we cannot step into a time machine, there is much we can never know about the social organization of Formative Mesoamerica." (*Ibid. :* 281). After making two speculative inferences, the authors suggest a tentative methodology for Mesoamerican archaeologists who are interested in reconstruction of Pre-Columbian social organization. These are listed here, as these might be useful for some of our studies.

1. Settlement pattern surveys aimed at showing whether the distribution of sites is "contiguous" or has a "balanced dispersal" with regard to resource areas.
2. The recovery of plant and animal foods in an effort to define specifically the products relied on, and the resource areas from which they come, with such a

study's implication for the nature of the economy : whether balanced reciprocity or redistribution dominates.

3. The investigation of house and even room patterns within villages where possible, searching for clues regarding the residence of corporate groups within the community itself.
4. Excavation of cemeteries in an effort to determine degrees of ranking, whether inherited or acquired, and the degree of status between members of adjacent dwellings and adjacent communities.
5. Utilization of ethnographic data from areas closely associated in time and space, where possible, with the prehistoric community.
5. Last, a search for correlations between residence patterns, economy, and social systems. This is the most difficult step, and the one which leaves us most open to error. (*Ibid.* : 281).

While it is not so difficult to infer about social organization and subsistence pattern about the late American prehistory (c. 1200 A.D.) because of the excellent preservation of the data, I was anxious to know how the newly oriented American archaeologist would face the problem while handling data from the prehistoric Western Asia. Here Frank Hole's paper on "Evidence of social organization in Western Iran 8000-4000 B.C." (Binford and Binford 1970 : 245) could have served as a model for scholars in India.

For the earlier period, 8000 B.C., frankly there is no data, and hence we find the familiar speculations that the natural environment had not been altered by man who was principally engaged in collecting wild plant foods, and hunting. What the former could be, he tells us from the studies of Dr. Hans Helbaek, but about social organization at this time, frankly it is purely speculative. Is it not the old way of inferring with regard to subsistence, social and political organization to say, "only a simple organization is called for" ? Since the flocks were *probably* property, some device of inheritance to keep them in the family must have been there. "The

amount of farming does not suggest a need for ownership of land". Hence he imagines, on the analogy of hunters today, a society composed of simple bands, with probably patrilineal lineages. (*Ibid.* : 249).

Later by c. 6000 and 4000 B.C., because of the evidence about pasture plants like plantain, and also cereal grains, some variation in life of the people might be inferred. But throughout the whole paper is based on "a chain of inferences", based on slender or no evidence of which Hole laments. (*Ibid.* : 251).

Passing from this to the discussion of "Settlement Pattern", Hole has many things to say which are true in India. He has a handful of excavations coupled with extensive surface samples, but no village site so fully excavated as to reveal differences in house plans, from which one could infer differentiation of status, and unusual function (*Ibid.* : 255). Hence his conclusion that "the Mohammad Jaffar people were living in autonomous villages composed of self-sufficient, socially equal people" is as much acceptable as ours about Inamgaon or Navdatoli. Perhaps the latter estimates would be found more reliable, based as they are on a fair sample of data. Even at Inamgaon site this season (1975), we found a five-room house.

Hole's views about the nature of the society, based on or drawn from the study of artifact variability are purely speculitive clues about the division of labour, about craft specialization, about the ritual or ceremonial activities of the Bus Mordeh society. Whatever he infers is suggested by what he has seen or read among "hunting poeple". Hence his conclusion that the society "was relatively undifferentiated and egalitarian" (*Ibid.* : 257).

The evidence for the next stage—Mohammad Jaffar Phase—is slightly more, but not positive (*Ibid.* : 258). Hence he sees no signs of "rank" or "status differentiation".

In the next phase, the Bayat people are found to use seals "which are usually interpreted as signs of individual ownership", and this could be further taken as a sign of "burgeoning status differentiation". By this Hole means a difference between

the producers and those who organized and directed production and distribution of the surpluses. (*Ibid.* : 259). However, no figurines or phallic symbols were found, and he therefore comes with the rather astonishing conclusion that perhaps "the personal do-it-yourself magic of earlier times had been replaced by a less personal, more formal ritual presided over by trained attendants". (*Ibid.* : 259). Thus one can go on speculating, but fortunately Hole himself realizes that "obviously we need far more data than we have, preferably positive rather than negative". (*Ibid.* : 260).

Stuart Struever's paper on "Woodland Subsistence-Settlement Systems in the Lower Illinois Valley" (Binford and Binford 1970 : 285-312) "investigates how the structure of material remains reflects the manner in which a social unit segments to exploit its biophysical environment". The problem was to find out what changes, if any, had occurred in the exploitative economy and the structure of settlement and social organization between the terminal, or Black Sand, phase of the Early Woodland period (c. 450 to 200 B.C.) and Middle Woodland times (200 B.C. to A.D. 400). (*Ibid.* : 289).

Two kinds of data were available : one from excavations and secondly from a survey of 30 sites. This was assisted by the previous study of the floral make up of the Lower Illinois Valley based on the kind, quantity and spatial relationships of potential vegetal foods. The reconstructed 19th century vegetation picture is treated as comparable to that of the centuries immediately before and after Christ. (*Ibid.* : 289).

"Black Sand living sites tend to be scattered along a series of low sand ridges situated in the Illinois Valley bottomlands and roughly paralleling the river." The sites appear to be of two forms : (1) small and scattered but with definable limits; (2) continuously scattered in a line. The surface survey showed that blades and handsize cobble stones for grinding were more than animal bones, mussel shell and burnt limestones. The last three were indeed scarce. These surface indications reew more or less confirmed by the complete excavation of a

site called Peisker. This afforded sufficient evidence for a hunting–butchering activity, pits for roasting; plenty of bones of land mammals, but little evidence of items of "ceremonial" nature, whereas burials, all within the site, gave no evidence of difference in status of the person buried.

Middle Woodland habitation sites are located entirely within the Illinois Valley itself; they do even exist several hundred yards outside the limestone bluffs that define its margin. The sharp increase from Early to Middle Woodland periods in the lower Illinois is interpreted as a change in settlement system, and an absolute increase in population size in the region. Relative population densities during the Early and Middle Woodland period may be measured by controlled sampling of all recognized micro-environments.

The evidence is supplied by the excavation of Apple Creek. However, it is said that it was not yet possible to demonstrate the association of specific artifacts and feature of tool kits, activity areas and areas of social distinction. However there are artifacts which give evidence of :

1. hunting and butchering
2. hammering and grinding
3. manufacturing of tools
4. recreation
5. socio-economic, i.e., objects symbolizing and communicating status of the person identified with the object, such as drilled bear canine teeth, pottery earspools and figurines.
6. structural features, deep, cylindrical storage pits and earth ovens, filled with burnt limestone chunks.

One complete house, 40 ft. in diameter, and the associated remains indicate that it served as a domicile. Subsistence practices were reflected in the nature and amount of animal bones. Carbonized plant remains were recovered almost entirely by water and chemical flotation processes. These methods yielded numerous hickory nut shells, acorn shells, seeds of *chenopodium*. Actual food remains showed major reliance

on deer, turkey, ducks, geese and fish. However, one of the largest sites, that of Mound House, though unexcavated, from its surface collection, appears to represent some sort of trade or exchange hub, and perhaps served as a centre of within and between region interaction, in which exchange activities took place articulating local Middle Woodland groups with each other. (*Ibid.* : 300).

The point of view enunciated by the Binfords and exemplified by some of their colleagues is further elaborated by Patty Jo Watson, Steven A. LeBlanc and Charles L. Redman in *Explanation in Archaeology*. The sub-title calls it "an explicitly scientific approach".

Two things are noteworthy in this respect. First, that these enthusiastic devotees of Binford do not fully accept the tall claim made by their *guru*. "Thus the claim is that the limitations on our knowledge of the past lie in the inadequacy of our research designs and methods, not in the archaeological record itself". Watson *et al* clearly regard "this as probably somewhat overstated". For they say, "even if all the material items of a culture are related to its non-material aspects, the archaeological remains may be so limited, altered, or destroyed that a complete description of the past cannot be reconstructed from them, not *just because our techniques (or intelligence) are limited,* but because the complete past simply is not reflected in the material that remains. The possibility of the material's being so limited that little knowledge can *be gained from it is certainly real,* but one cannot allow the possibility to prevent him from attempting to work out all aspects of past human behaviour". (Italics mine)

This is exactly the position. It could not have been more clearly and honestly stated. Except the late American sites of which elaborate descriptions are given (see above), and which can be regarded as prehistoric by courtesy only in a country where everything earlier than 1600 A.D. is prehistory, this deductive-inductive method cannot be followed in all archaeological investigation—whether it be excavation or exploration.

This is certainly a worthy goal in all human endeavour,

But how far is it practicable in archaeology which by definition deals with any part (surviving) objects ? Most of the examples discussed in *New Perspectives in Archaeology* deal :

(i) with late prehistoric sites, where the archaeological record has remained *intact* or survived,

(ii) with cases, where ethnographic parellels are still available and hence the deductions made from the archaeological study can be tested.

Where all these possibilities exist, there some (greater) objectivity in our approach can be achieved by Factor Analysis and other new scientific techniques in analysing the data. Some of these have been given in the second part of this (Watson's) book. Meanwhile, we should further point out the limitations of ethnographic or anthropological data in arriving at our deductions.

The tall claims of 'new archaeologists' that archaeology should be explicitly scientific has been admirably questioned by C. G. Morgan, Assistant Professor in the Department of Philosophy, University of Alberta (Morgan 1973 : 259-75). As Morgan points out, this concept of Covering Law model (called C L model) has been taken from Philosophy. Hence, using the standard terminology, "A description of the event, happening, phenomenon to be explained is called 'Explanandum', the information cited in an attempt to explain is called the 'Explanans'. The CL model purports to analyse the structure of the explanans and its relationship to the explanandum." (*Ibid.* : 260). It is further thought that the C L model has a prescriptive force—anything which does not fit the model cannot be an explanation. Further it is universal in its application. Morgan then discusses in detail the C L model and its application. By way of criticism he points out that the authors have presented their case very inadequately. No examples are given. Thus Watson *et al* "seem to be equating deductive-nomological with deterministic, and both of them with causal" (*Ibid.* : 267). Thus the example given by them (*EA.* : 8).

1. Women are the potters in this society.

2. Pot designs—motifs and their placement—are handed down from mother or grandmother to daughter and granddaughter with innovation or copying from another than mother or grandmother being very rare.

Now both these statements do not have (at least superficially) the form of the laws given in *EA*. "In fact they look more like statements of particular facts or at least low-level accidental generalizations." (*Ibid.* : 268).

Further, their discussion of the testing of hypothetical explanation is rather confusing. Thus to cite the example given by Watson *et al.*
Considering the work of Longacre on the Carter Ranch Pueblo, they quote :

"If there were a residence rule which led to the related females living in the same locale through several generations, then ceramic manufacture and decoration would be learned and passed down within the context of this residence unit (assuming female potters). (Longacre, in Binford and Binford : 1970 : 97).

They proceed :

"Treating this assumption as a law he (Longacre) formulates a hypothetical explanation with the conclusion that there was such a residence at Carter Ranch Pueblo: Related females lived in the same locale through several generations". (*EA*. : 35).

This might be put in the form of a law as follows :

C ("societies with female potters").

A ("residence rule which led to related females living in the same locale through several generations").

B ("ceramic manufacture and decoration were learned and passed down within the context of this residence unit").

Put in logical form :

In C, if A then B,
A case of type C is being considered
B

Therefore, A.

This argument is not deductively valid. That is, thus the explanandum cannot be deduced from the explanans. We commit logical fallacy of affirming the consequent which occurs when one argues from 'If A then B', and 'B' to 'A'. Hence the so-called law would not fit the C L model,

A valid argument would be as follows :
In C, if A then B
A case of C is being considered.
A
Therefore, B.

Though logically valid, the reconstruction does not follow directly from the archaeological remains. Further detailed consideration shows that "the example given by the authors of *EA* does not fit the C L model." It is an attempt to support a hypothesis by making a plausible (but not deductively valid) chain of reasoning from the hypothesis to observationally available data." (Morgan 1973 : 272).

Morgan further points out that there is a repeated appeal to authority and sloganeering. His conclusion is worth quoting. ' "In short, *EA* seems in many places reminiscent of a religious revivalist, appealing to scripture to establish his points while surrounding his doctrine with flowery phrases and redefinitions to make that doctrine more palatable". (*Ibid.* : 273).

What should then archaeologists do ? They should continue to report the types and distribution of artifacts, which they find at their sites, for such information will be the basis on which we must test the speculations, if they are so inclined to formulate and test such hypotheses. "New methodologies, particularly the use of statistical methods such as factor analysis should be incorporated once *their usefulness is established.*" (Italics mine)

Morgan then says, in spite of all this sloganeering, "the work of the "new" archaeologists seems to be limited to the establishment of features of some specific society or culture. No law of cultural change has yet emerged, and large gaps in our knowledge of past cultures and how they evolved and changed still remain. The establishment of such laws is vitally dependent upon the work of historically oriented archaeologists. Archaeologists should not, therefore, be discouraged from taking the historical points of view in their work". (*Ibid.* ; 275).

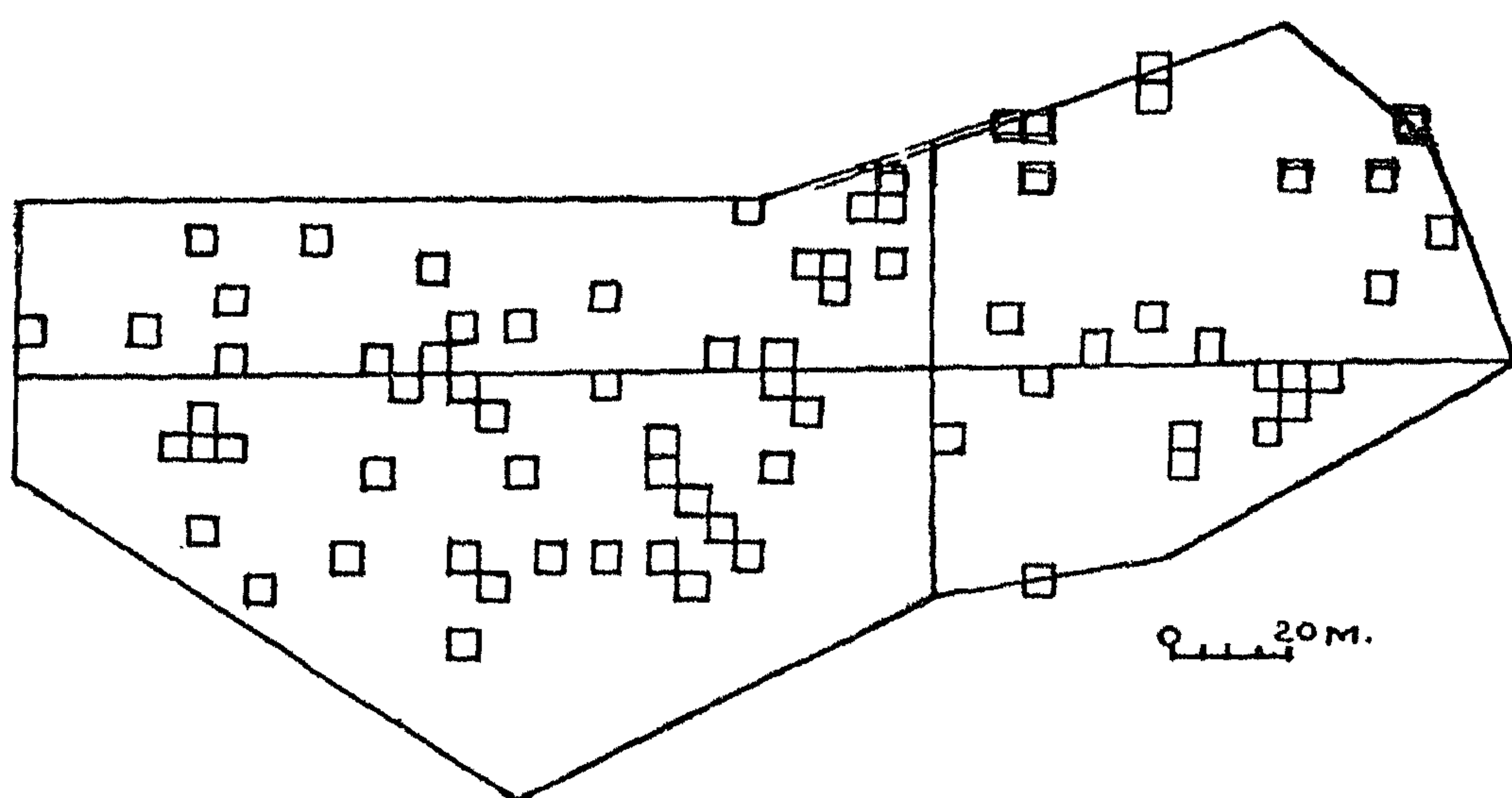


Fig. 2. Outline map of archaeological site of Cayonu, Turkey

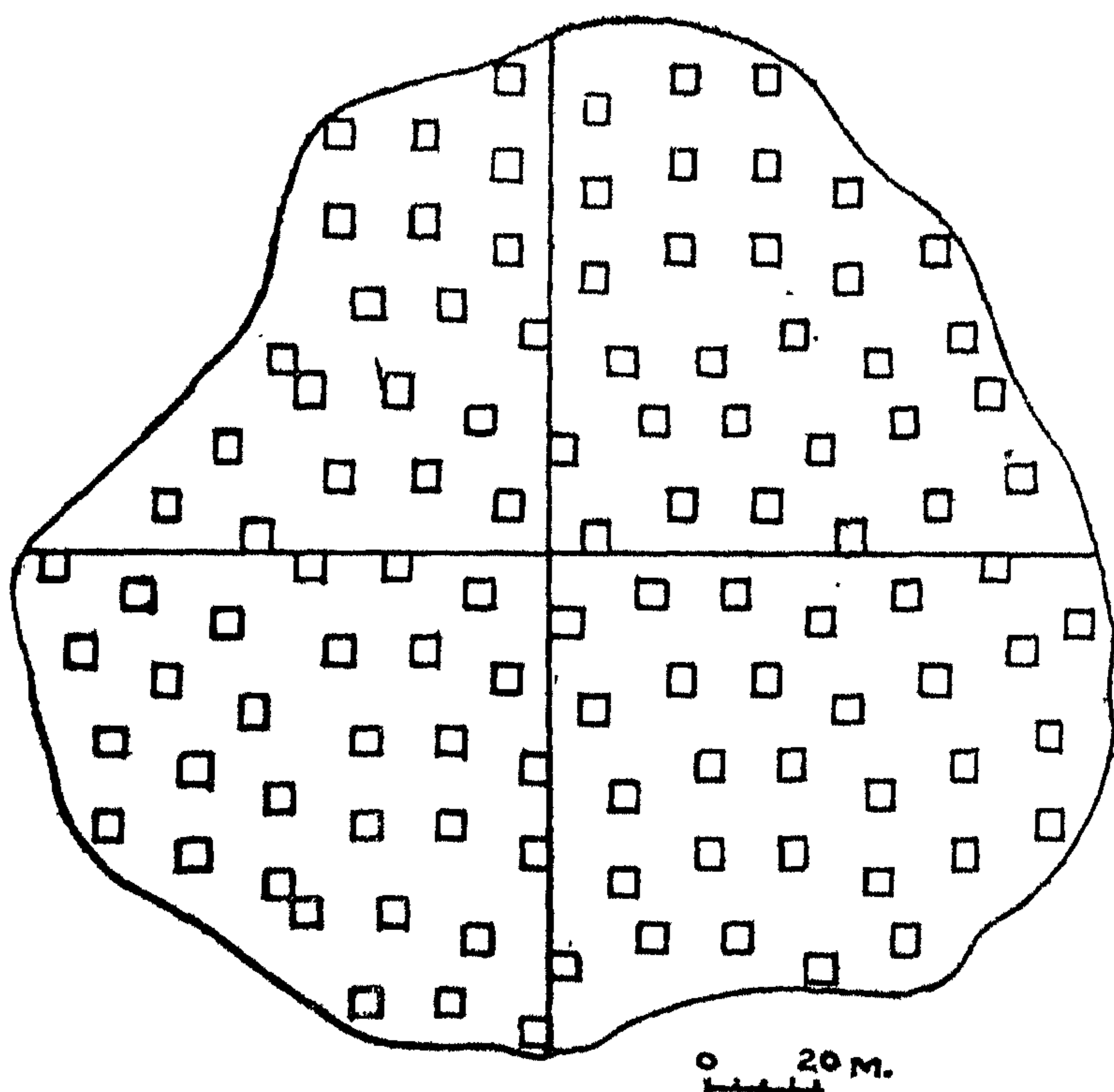


Fig. 3. Outline map of archaeological site of Cayonu, Turkey

[p. 58

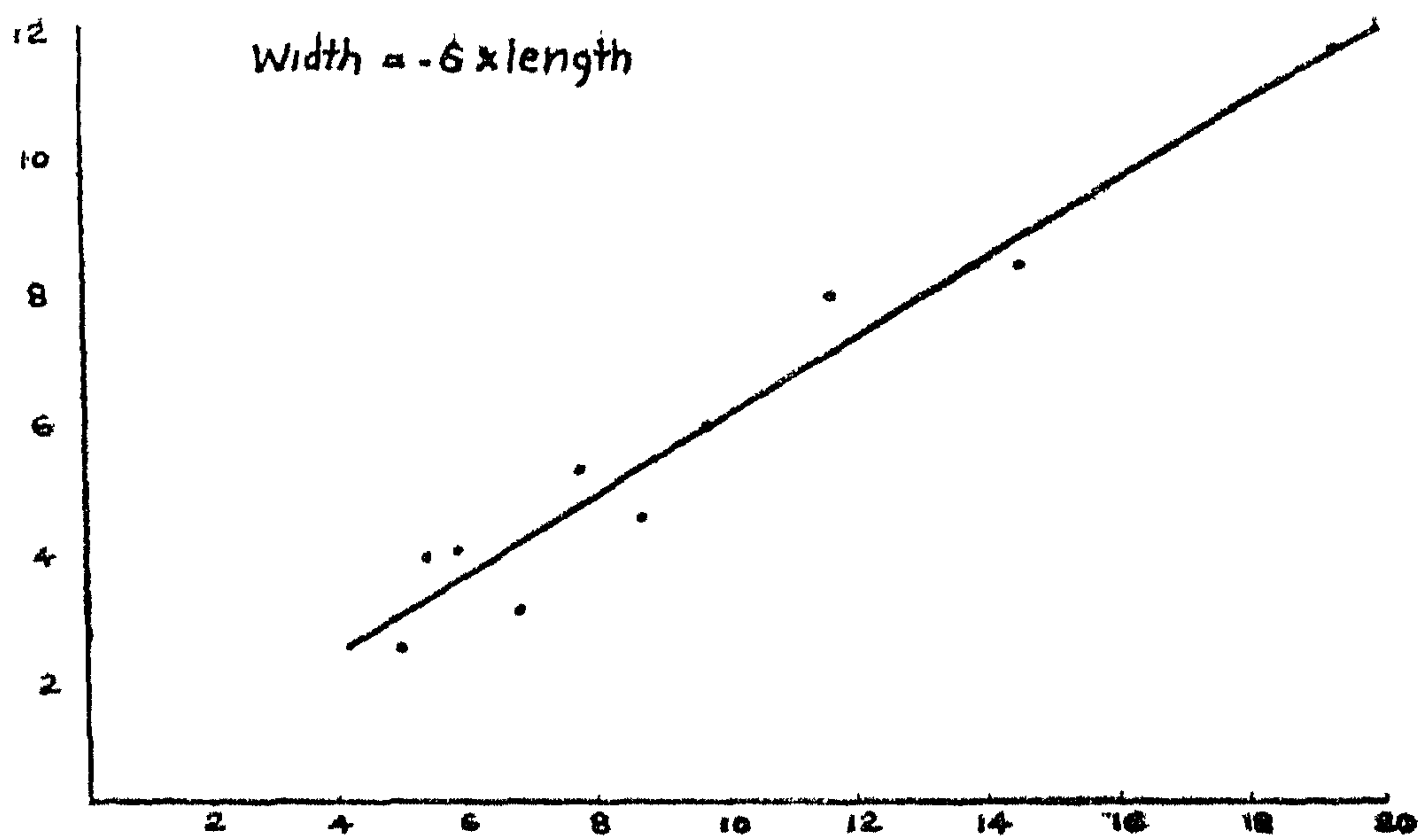


Fig. 4. Regression graph of 10 Projectile Points

[p. 58

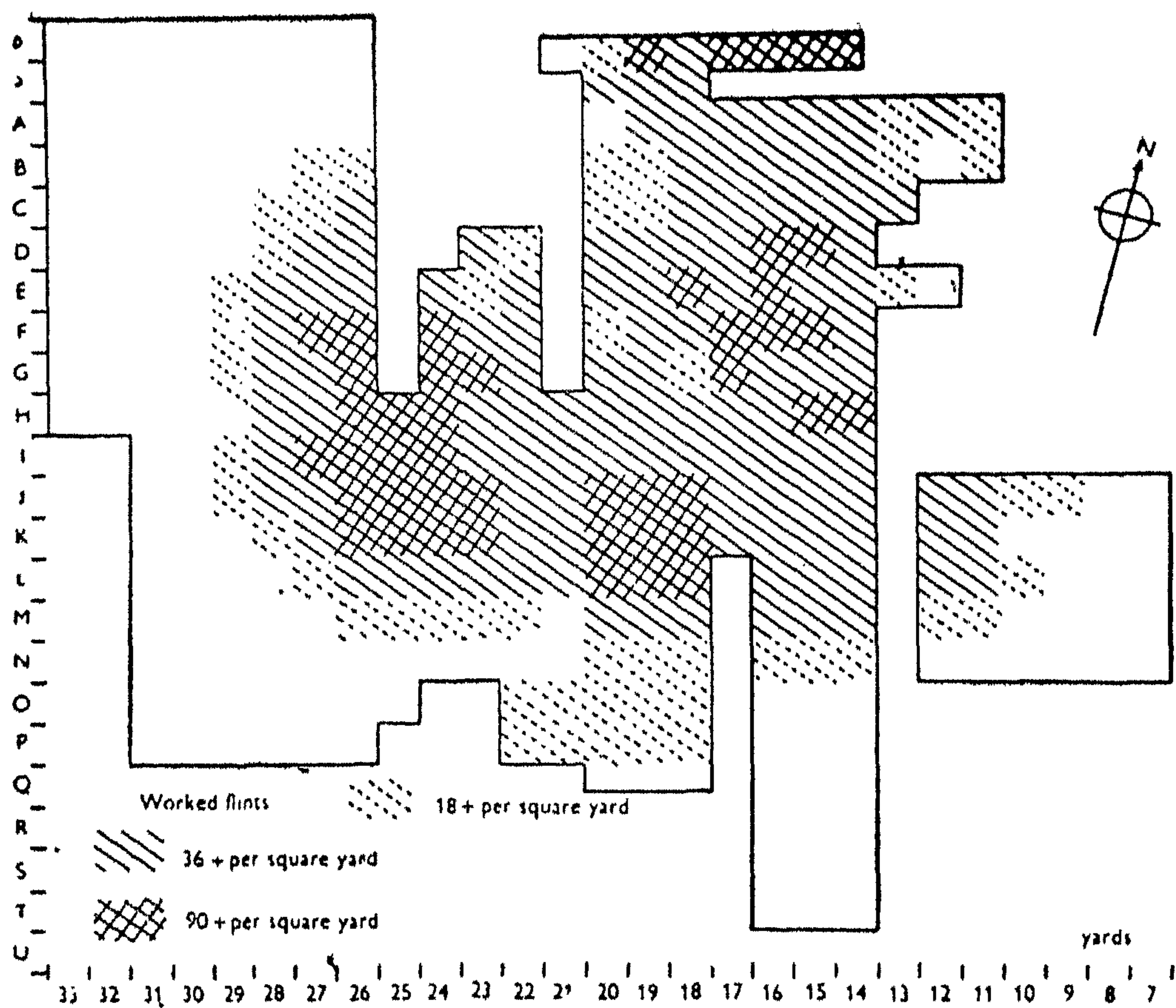


Fig. 5. Plan showing the distribution of worked flints in the Mesolithic Settlement at Star Carr, England

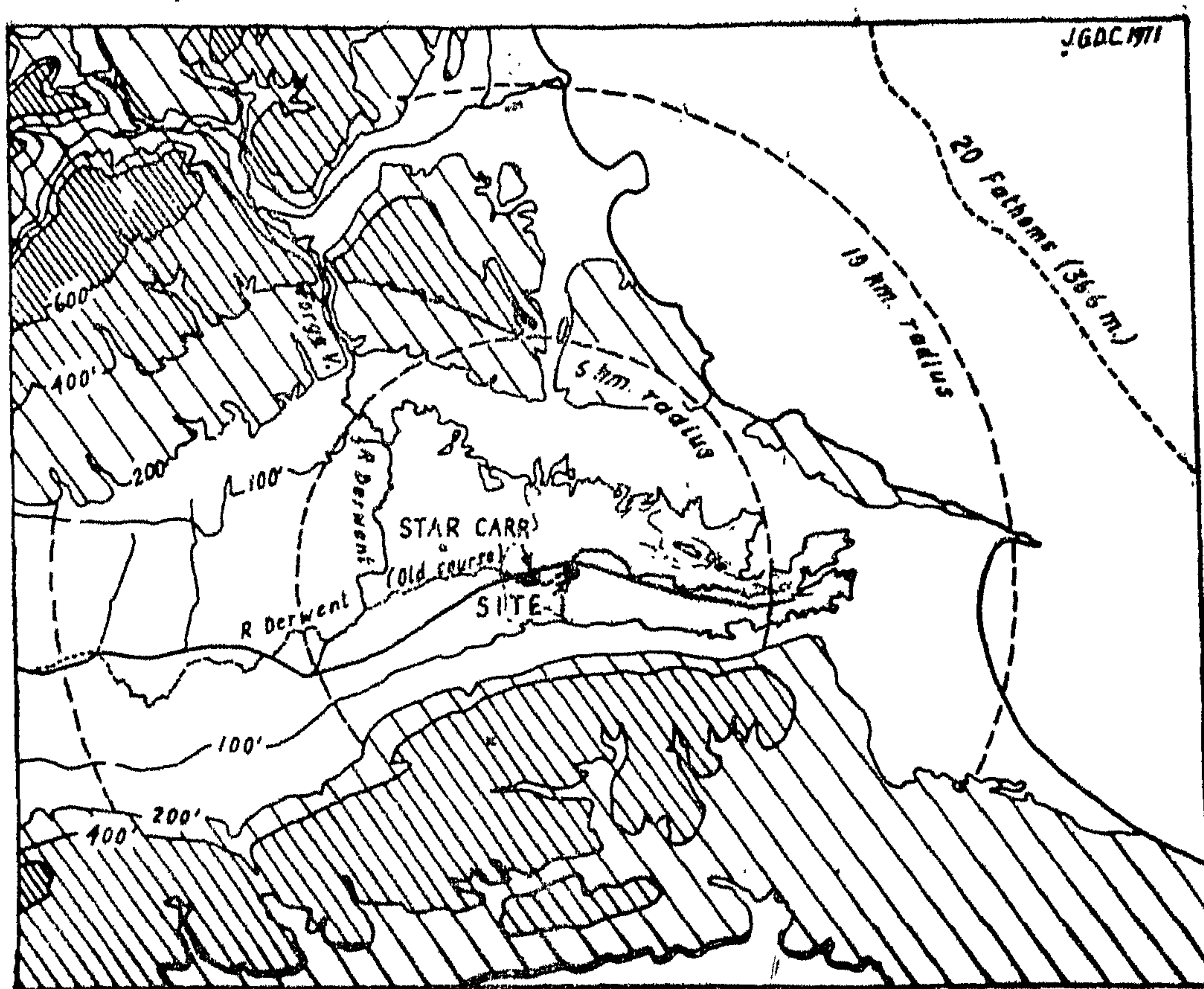


Fig. 6. Map showing the probable range of Red Deer grazing at Star Carr, England

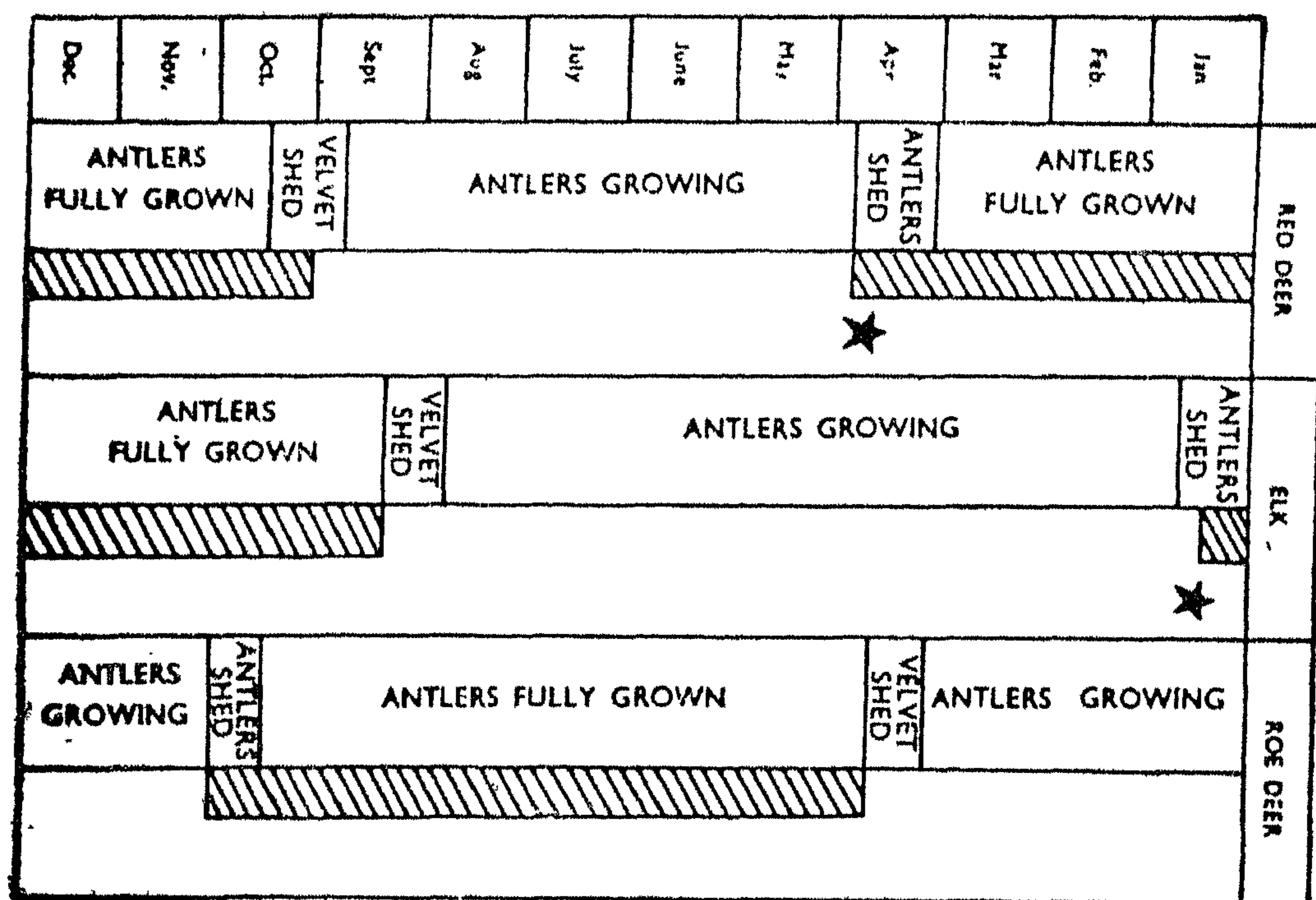


Fig. 7. Chart showing the quantity of antlers in various months at Star Carr, England

New methods & new concepts

In order to have an overall or as many sided view of culture, new archaeologists have taken recourse to several new techniques and concepts. These arc briefly discussed and illustrated.

Normative view of culture

Culture is (or was) defined "as a body of shared ideas, values and beliefs—the norms of a human group. (Flannery 1967 : 119; Watson *et al* 1971 : 61). Hence an archaeologist's task lay "in abstracting from cultural products the normative concepts extant in the minds of men now dead". (Binford 1965 : 203). These normative concepts or "mental templates" served as models for the prehistoric man who made the artifact. It is assumed that the archaeologist's task was to discover inductively the ideas that governed the production of the artifacts, and in this way to get at the essence of the culture. The emphasis on artifacts as reflections of shared ideas as the essence of the culture has led to the incorporation of the coneept of type sites—implying thereby this or that site typifies all sites of that period and region. But, according to Watson *et al* (1971 : 62) there is a danger in such an attitude. "Concern with type collections and typical sites results in ignoring a great deal of variation that is actually present in archaeological material". This has resulted "in a crisis in archaeological interpretation." Final recourse in formulating explanations must be to historical accidents, such as migration and diffusion or to what Binford calls "palaeopsychology." This is because the normativists (archaeologists) do not deal with relationships between norms but attempt to explain the norms themselves. "Because these norms are regarded as independent forces and not as functional aspects of the culture, one can only describe them and their travels or distributions". It is said, "these descriptions provide little information about the archaeological remains which would enable us to test processual hypotheses concerning the extinct culture". (*Ibid* : 63).

Instead of a single explanation of a change—say pottery form and design of the Jorwe culture and the preceding Malwa culture at Inamgaon—as due to time (chronological proximity), the change might be due to several other factors, such as geographic distance between settlements or the pressure of different social groups within a community. "There is a shift from exclusive concern with entities to interest in the relations between and among groups and entities. "An archaeologist interested in such relations (possible laws of cultural process) cannot search for the 'typical' site and stop there; he must attempt to discover the range of variation in the prehistoric record and to interrelate the causal variables which resulted in its present form". This is called the systematic view of culture with its multivariate approach and emphasis on relationships and variability (Watson *et al*: 65).

Models

There are two kinds of models, traditional and new.

The former has two: a) Mechanistic
b) Organismic functional

The mechanistic model treats culture as "machine" and has been borrowed from engineering. In this the output always bears a particular relation to the input. For example, an increase in the presence of warfare in a region leads to an increase in the construction of defenses. The various parts of a mechanistic system work together in harmony to produce the predicted results. In this model no allowance is made for change or dynamic regulation in the face of adversity; hence it is regarded as inadequate for a study of culture change.

"The organismic or functional model has been borrowed from biology. The basic concept is that each element of the system is interdependent, works together, and functions to maintain the operation of the total system. Though an organism grows and develops according to a predetermined plan, still it is able to adopt to certain changes in the input

from its physical surroundings by employing sensing and regulating devices within its system. Although the organism is capable of adapting to change, this change must be within the limited boundaries. Successful adaptation means the organism alters its interaction with the environment in order to maintain its original form in a milieu of changing surroundings". (*Ibid* : 67).

"This framework has proved to be very useful in interpreting data from systems that are not changing significantly, and in understanding how parts of systems work together to adapt to small changes in the environment."

Example

Increased warfare in a region could be considered from the perspective of an organismic model. Plausible responses expected by the investigator might then include the increase of its outer shell (defenses), movement to another enviornment (resettlement), or even development of a new element (such as a strong army of its own) to maintain its static state. According to new archaeologists these "responses" cover a certain range of reality, but by no means all the possible alternatives open to a socio-cultural system. On the other hand "the system approach promises that it will enable archaeologists to get at the full complexity of the interacting phenomena of cultural process."

"The system-oriented archaeologist should not be interested in artifacts or activities in themselves, but should seek out their internal relations within a system and work out the way this system behaves in a given environment. The traditional analytic technique for studying relationships has been to isolate pairs of variable and to study their behaviour." "This technique is no longer regarded as adequate because in most systems pairs of variables do not act in isolation nor do they perform a constant task". (*Ibid* : 69).

Systems Terminology

System theorists have developed or introduced some basic

terms and concepts, and tried to illustrate them graphically with diagrams. I do not refer to these because I regard them at the moment too difficult for understanding. (*Ibid* : 69-83).

In the section on "Systems Perspective" (*Ibid*: 83 ff) Watson *et al* once again state the importance of investigating *all* the different forms and functions of settlements during each period. Sampling within a site must include all aspects of the site in order to define the range of activities performed there, as well as their configurations and demographic parameter sampling. Artifacts must be categorized and analyzed in a manner that enables their variability to be compared along many different dimensions, clustering and grouping artifacts into "types" is a useful and necessary procedure for some kind of analysis, but not for a systematic analysis. It might be better to measure all of the relevant variables of the artifacts and consider the distribution of each of these variables independently before clustering them into types. This *gives the researcher* more than one dimension along which to measure *artifact* variability (*Italics mine*). This helps, according to Binford, the separate (and independent) comparison between technical, morphological and decorative dimensions of artifacts. (Binford 1965; Watson *et. al.* : 84). Normally, in the older method, it was customary to explain a culture, or chronological change by isolating one, two or more variables and to observe their variation over a period of time. This may produce a developmental sequence, but according to the new school of thought, does not give or yield an accurate description of a process. Thus in the Stone Age assemblages, we take for comparison, say a particular type of handaxe, say an ovate type, and try to measure its development either stratigraphically, or in space and time in a wider context; or take for instance, the dish-on-stand in the Harappan pottery or a stud-handled bowl in Saurashtra sites. In all these three cases we certainly do see a change from one sub-phase to another. According to the new archaeologists these changes, either in the ovate, or the dish-on-stand or a stud-handled bowl do not

explain accurately the description of a process. What the archaeologist should examine is their position, say the ovate in the handaxe culture or site (camp or factory) or dish-on-stand in the Harappan. Thus we should see whether the object under study maintains the same position within the system during each of the periods.

Here the preconditions for such a study should be mentioned. This is that the site under study must have been excavated on a horizontal scale (however limited in extent this excavation is). That is, this kind of analytical study has little scope where the evidence is from a vertical excavation. For then there is no possibility of understanding the contextual significance of the object under study. At the most some idea can be had of the stratigraphical (and hence chronological) position of the object. And that is what has been done over large parts of Indo-Pakistan continent during the last 25 years.

It is claimed that the systems theory's greatest contribution to archaeological research is in the formulation of testable models of human behaviour. However, this tall claim, as well as the further claim that "the central purpose of a theoretical model is to aid the researcher in the selection of relevant variables, and significant hypotheses from an infinite number of possibilities" are to be accepted with a pinch of salt. For, as will be pointed out further, an archaeologist has always to work with an admittedly inadequate archaeological record. And even if the site is found intact with all the things in a house left as they were, still an ethnographer archaeologist is likely to commit errors in his estimation, as has been vividly illustrated by Millie's camp study (Bonnischen 1973 : 277-291).

Watson *et al* incidentally also refer to some sub-disciplines within the systems theory, such as "Information Theory" and "Games Theory", but regard them as a less useful descriptive tool and as a model builder (*Ibid* : 86).

Ecological view of Culture

Among the various systematic frameworks, the ecological

view of culture is now receiving more and more prominence. For, the item of cultural behaviour which an archaeologist or an anthropologist gathers during his investigation can be explained as part of systems that also include environmental phenomena or they show that the environmental phenomena are responsible in some manner for the origin or development of the cultural behaviour under investigation. This means, say Watson *et al*, "that the relationships between the cultural realms are either functional or causal" (*Iibd* : 89). Because of this new perspective, the emphasis both in archaeolagy and anthropology is not on objects but on the understanding of the relations. "The artifact is no longer seen solely as an object with importance of its own, but as a mediator between man and his surroundings". "This is a very practical approach for archaeologists in that it embodies several categories of independent data on topography, flora, fauna, and natural resources used that are reasonably easy to infer from the archaeological record. Hence an ecological approach leads to many testable hypotheses concerning prehistoric cultural systems" (*Ibid* : 89).

Statistical Techniques and Archaeology

For various reasons statistics have been increasingly used in archaeological studies. Its use starts with the formulation of the research design (for instance sampling) to artifactual analysis (typology), defining relationships between artifacts (factor analysis) to the testing of hypotheses about cultural process.

These techniques have three general uses in archaeology.

1. They provide an efficient means of representation of the data.
2. Statistical inference is an aid to model building, that is to formulation of hypotheses or complexes of hypotheses.
3. Statistics can be used for testing hypotheses.

Statistical methods are related to scales, or means of measurement, used in recording the data to be analyzed. There are three kinds of scales : (i) interval, (ii) ordinal, and (iii) nominal.

Interval scale

This is the most familiar; it refers to any type of measurement with fixed and equal intervals, and equal intervals between the points of the scale.

Examples of such scales are interval scales as provided by rulers, thermometers, stop watches, etc. These measurements are to be taken on interval scales.

In archaeology, we may cite blades used during the Chalcolithic Period in India.

(a) The smallest one may be one inch in length.

(b) The middle one may be two inches in length.

(c) The largest one may be three inches in length.

We thus know that type (c) is 2 inches longer than (a), and type (b) one inch longer than (a).

"An interval scale embodies the maximum amount of quantitative data about these lengths, and in general, the most powerful statistics are those that can be used with the data collected on interval scales".

Ordinal or Rank Scale

The second kind of scale is the ordinal, or rank, scale. It allows one to compare items with one another but only on a relative scale, as in Moh's Hardness Scale for minerals or Munsell's for colours.

Thus if we decide to rank the Chalcolithic blades mentioned previously according to length and assign them numbers 1 to 10 we know that a blade given a value of 9 is longer than one given a value of 8, but we do not know how much longer it is. We cannot assume that there is any absolute amount of difference represented by the units of the scale.

It is clear that the ordinal or rank scale embodies less quantitative data about the blades than interval scale. Hence the statistical techniques that can be applied to ordinal scale are weaker than those that can be used with interval data.

Nominal Scale

"The third is the nominal scale. This scale merely places

items into discrete categories without assuming anything about the differences between categories. A nominal scale might have divisions based on shape, colour, religion. Any scale that measures simply the presence or absence of some attribute is a nominal scale. Thus the blades when classified into "long" and "not long" would be according to a nominal scale, and as such it conveys less quantitative data about the blades than do the other two scales. Interval scale provide the greatest quantity of statistically meaningful information, but tabulating data by interval scales is usually much more tedious and time consuming than tabulation using ordinal or nominal scales". However, it is also realized that archaeological data is either inadequate, or vitiated due to various reasons—natural and man made—that one should be really pragmatic and careful before employing statistical methods.

Seriation

"Seriation makes the assumption that when a number of assemblages of different date are found in the same region, those which are most closely similar will be closest in date. On this basis, it is possible to arrange the various assemblages in serial (i. e., chronological) order by following routine procedures. The numerical calculations are exceedingly simple, but to try the various possibilites and to choose the best ordering involves almost endless repetition. Here the computer is used in its role of a high-speed moron". (Renfrew 1968 :236)

In this attention is drawn to Flinder's *Petric Inductive Metrology* published in 1877 and Oliver Myer's *Some Applications of Statistics to Archaeology*, Cairo, 1950 by Thurston Shaw and S. G. H. Daniels (*Antiquity* 1969 : 147).

Some technical terms

Dimension : "A dimension is some formally defined aspect of the group of objects that is being studied....Every dimension is measured in terms of a scale, and the various mutually exclusive positions of the scale are attributes"....The

attributes of interval scales are defined by the establishing of the manner in which the measurement is to be made. Once this is done, the attribute is merely the value of the measurement, for example, dimension, length; altitude: 10.5 cm. For nominal and ordinal scales, mutually exclusive attributes must be defined so that the chosen aspect of any of the objects under investigation can be classified as belonging to one and only one of the attributes of the dimension. For example, if we measured the dimension of chalcolithic blade's length and wished to have two groupings, we could define the attributes as "short" and "long", but not "poor" or "bad" or "good". (After Watson *et al.* : 138-39).

Chi–Square Test

To determine whether two dimensions are independent, one can use the Chi–Square Test. This is one of the most useful statistical tools available to archaeologists. The Chi-Square Test shows the probability that the observed distribution of the data is due to chance alone. One can use the Chi -Square Test when a set of items has been cross-classified by two dimensions. For example, in an excavation of a grave yard, it was found or observed that in 33 cases out of 60, red pots were placed with males and in 7 cases the pots were not red, whereas in the case of females the proportion was 12:8.

In the end it is left to the individual concerned to set a high or low level of significance. In practice, a real problem does exist, and one must weigh the advantages between two alternate possibilities.

Thus some personal bias does play a part, and this is particularly so in archaeological data where "there are many gaps between the behavior of the people who once occupied a site and our description and interpretation based on the altered and selected debris that resulted and remains in the archaeological record from that behavior." (*Ibid*) "This does not mean that the data are unusable but it does mean that statistical procedures must be modified as required." (*Ibid* : 144).

"Correlation and Regression analysis"

How should one know about the nature of the relationship when it is decided that relationship between two or more dimensions is not due to chance ?

Analyses of this kind made on two dimensions are called "correlation analyses" and "regression analyses". If there are more than two dimensions to be considered, the techniques are generally called "multivariate analyses". Some of the techniques of multivariate analysis are multiple correlation analysis, multiple regression analysis, and factor analysis.

Correlation analysis can be done with interval, ordinal and nominal scales but different statistics must be used for each scale. Regression analysis can be done with interval scales alone. It shows the form taken by a relationship between two dimensions and enables one to derive an equation of this relationship. In this sense it is a predictive statement, because given a value of one dimension, one is able to predict (within a certain margin of error) the value of the other dimension. Regression analysis can be demonstrated by first plotting all the values of the two dimensions on a graph, then finding a line that minimises the square of the distance of each point from the line. A straight line is usually found, so that this technique is called linear regression analysis.

This can be explained by an example as follows. In America there is an artifact called "projectile point". Professor Spaulding wanted to study whether the width of this point bore any relation to its length. So he measured the lengths and widths of these points on an interval scale and plotted the results. "One can determine the regression and plot it, all on the same graph. This particular regression equation, width $=.6\times$ length, shows that for every centimeter that length increases, there is. .6 cm of increased width. For any length, one can predict what width should be found. It is possible to draw some sort of line through the points on the diagram even if they are randomly scattered over the graph, so one needs to know how closely these points fit the line. That is really

asking whether the dimensions are related or independent."

This is answered by using a measure of correlation and other statistical methods. But, it is "also noted that a strong degree of association does not demonstrate a causal relationship between dimensions". (*Ibid* : 147).

Multivariate Analysis

Multiple factor analysis, or simply factor analysis, is a technique which "begins with a large number of measures and reduces them to a few hypothetical basic variables. First, data are recorded by means of a series of dimensions, then the values of each dimension are correlated with those of every other dimension. It is then assumed that there is really a smaller set of variables that can explain correlations observed, because the original dimensions are probably each measures of several different variables at the same time. By finding these new variables, which are labelled factors, one simplifies the empirical situation. These factors can produce a difference in representations that is easier to comprehend" (*Ibid* : 148). Hence they are used by archaeologists.

"Fewer factors than the original number of dimensions can usually account for most of the original correlations, but not all of them. The portions of the correlations unaccounted for are labelled 'residuals" (*Ibid*).

Next comes the interpretation of these factors. In this a process called "rotation" is employed. If it is assumed that the observed correlations among the original factors were independent of one another, then the factors could be grouped at right angles to one another. Hence the factors are called "orthogonal". Even after "rotation", one must still interpret the factors and decide just what effects or causes they represent. Finally what factors really measure is logically on the level of hypothesis formulation. And these hypotheses should be tested by independent means. (*Ibid* : 149).

Orthodox archaeologists and culture historians naturally get frightened at all these mathematical calculations, the time

it consumes and almost unintelligible data it all gives. The first person to voice concern over this development was Mrs. Jacquetta Hawkes, one of the authors of the UNESCO's *Cultural History of Mankind.*, when she found that Dr. McBurney had taken 10 years in preparing his report on a small vertical excavation at Haua Fteah and it contained 250 graphs and statistical tables for a reliable comparison of McBurney's material with other sites in the area or elsewhere, but still emphasising the need of a firsthand study, "the other man's actual material in the old-fashioned way". And she wondered whether the archaeologist was led "to participate in the prestige of the natural sciences and put up a scientific facade far too grandiose for the modest historical building behind it." So in the end, she said "the need of the age is to protect our humanity, to keep our technological Frankenstein's monster in control. Archaeology with its revelations not only of our humble beginnings but still more of our marvellous variety of achievements, has, I believe, a considerable part to play in this defence. If we are not to fail in it, we must remember that all our ingenious devices, all our exact measurements and statistical analyses are of no value in themselves". These must not dominate our subject, lest they repel young men and women with strong historical imagination (Hawkes 1968 : 255-262).

The subject, "Whither Archaeology?" was further discussed for some years in the pages of *Antiquity*, scholars like David Clarke arguing in favour of the new techniques and even new jargon. But when the President of such a prestigious institution as the Society of Antiquaries, London wondered whether Dr. David Clarke's English was acceptable, the *Antiquity* stopped the discussion. (1973 : 95).

It is indeed interesting to note that though David Clarke did not like the President's characterization of his language as "gobbledygook", still the language he has used in his review of R.R. Newell and A.P.T. Vroomans' book *Automatic artifact registration and systems* for *archaeological analysis with Philips* P 1100 *Computer : A mesolithic test case* (*Antiquity* 1973 : 159-60) is almost without unintelligible jargon. The criticism has had its effect.

Whether we agree with all these *obiter dicta* of David Clarke, or the tall claims of Binford, Watson and others, still there is no doubt, as Professor Grahame Clark who has, like myself, passed through the three phases—viz. Typological, Stratigraphical and Functional—in the study of the Mesolithic and other archaeological deposits, has said, "If prehistoric archaeology is to contribute to anthropology as a serious discipline, it can do so only if its practitioners are ready to take account of conceptual as well as technical advances and sometimes the adventitious accumulation of data. The importance of technical development stemming from empirical observation and of the mere accretion of new facts should not be minimized. Yet the progress of prehistoric archaeology as a discipline depends fundamentally on improvements in the quality of the concepts and theories which guide research and mould its interpretations".

And not only Professor Clark believes in such improvements but has also tried to review his own work—viz., *Excavation at Star Carr*, a small Mesolithic settlement in Yorkshire, and by adopting several models, has given us an insight (of course sometime speculative) into the life of these people. Thus we now know about the probable unit of the family and the number of families which could have lived on this small marshy land, the likely protein content in their food, which was largely meat, and the probable range of their activities, as well as those of the animals—red deer, etc.,—whom they hunted, and lastly, the probable population of England at that time (c.9000 B.C.). (Clark 1972 : 1-42).

It is in this spirit that I have been, for the last 10 years or so, before Professor Clark published his re-assessment of *Star Carr*, looking on the Indian scene, and trying to extract some more significance from the excavated data, as well as standing monuments of the historical period.

Interestingly, it is really a question of "independent" development as opposed to "cultural influence" or "diffusion", for I had not read anything of the American material, nor even

heard of the name of Binford. But what the new archaeologists wanted, and have sought to do, I attempted to do with the large material, over 23,000 lithic blades from our excavation at Navdatoli. After over 2 years of study, examining *each* intact blade, and relating it to the excavated house remains, I could prepare a paper on the "Socio-economic significance of the Lithic Blade Industry of Navdatoli." An abstract of this was published in *Current Anthropology* 1967 : 262-68.

About the same time, I visited Khajuraho, and saw the beautiful temples for the first time. Curiously, I could think not so much of their artistic beauty, as of the socio-economic conditions which could bring about this unique artistic creation in a forested environment.

It is this point of view that I have been not only advocating but trying to adopt in our excavations at Chirki, Nevasa, and Inamgaon.

Taking advantage of this occasion, I have tried so show in second part of these Lecture Series, how we could profit by some of the new concepts, methods and techniques, not only in the prehistoric but even in historic archaeology. For our aim should be to advance our knowledge of the past by all possible means. Hence no avenue is to be left unopened or untrodden.

● ●

Lecture II

We have some idea of New Archaeology, its aims and goals, its methods and techniques. We have now to see how far these are or can be made applicable to India, and what results we may expect from this new approach.

Archaeology in India

For a systematic consideration we have to examine

1. the nature of our sites
2. the methods followed in their excavation
3. results obtained so far.

Archaeology in India, unlike that in the U. S. A., has a long and rich past. It is broadly divisible into three periods

1. Prehistoric
2. Protohistoric

3. Historic

The prehistoric roughly covers a period of about five lakhs of years, say c. 500,000 *B. P.* to 5,000 *B.C.* and includes all the Stone Ages—Early, Middle and Late. The Protohistoric Period again roughly covers a period of about 5000 years, c. 5,000 B. C.–500 B. C. and would include the various pre-Harappan cultures of Sind, Panjab, and Baluchistan, the Harappan Culture or the Indus Civilization, and the post-Harappan Cultures in different parts of India.

We shall not discuss in detail how these were excavated. For our purpose it would be sufficient to illustrate how the famous sites of Harappa and Mohenjodaro were excavated, and with what aims and results, and then see how the results could have been different, if the excavators had adopted new aims and methods.

In this appraisal, one must remember the state of our knowledge some 50 years ago, and the nature of our sites, and the general level of archaeological methods in the world.

In 1921 when the sites of Harappa and Mohenjodaro were discovered, very little was known archaeologically about the history of India, say before Asoka, that is before the 3rd century B. C. Hence the desire and natural emphasis was on the discovery of some pre-3rd century B. C. objects or antiquities.

Excavations at Harappa

So at Harappa or Mohenjodaro the first and the main aim was to get antiquities. In discovering these antiquities, only some idea of the depth at which these antiquities were found was noted in the old fashioned way so fittingly described by Sir Mortimer Wheeler.

Secondly, compared to the sites in the U. S. A. or even in Western Asia, these Indian sites were enormously big; they also covered the debris of several periods or phases, even if the general period be the same. Even if the excavators wanted to follow the methods followed today, they would not have

succeeded, as Wheeler has admitted frankly, in laying bare the traces of an extensive civilization. So considering the time, particularly the level of archaeological concepts and methods then obtaining in Western Asia, and even in the U. S. A. and much of Europe, excepting Britain where after General Pitt-Rivers' stratigraphical method of excavation and three dimensional method of recording the finds was adopted, and refined by Wheeler, we have to say that what was done in India by Marshall and his colleagues was not so bad. But the things could have been better and much more significant if some of the new concepts could have been thought of and applied. These in fact could be applied with considerable profit in the excavation of Harappan sites, like Lothal, Kalibangan and Surkotada, or those dug in Pakistan. We shall illustrate this point by citing a few examples from Marshall's *Mohenjodaro and the Indus Civilization.*

Religion : Linga worship

Our knowledge about religion or cults and religious practices in the Indus Civilization rests upon insecure foundation. As is well known, the prevalence of the *Linga* worship at that time (in Harappa and Mohenjodaro, and possibly elsewhere at Periano Ghundai and Moghul Ghundai in Baluchistan, rests upon inferences made by Marshall from three kinds of aniconic objects. These are generally of stone but occur occasionally in other materials as well.

(1) The first class comprises those specimens illustrated in Pl. XIII, 3, and XIV, 2, 4, 5. Of these, two (Pl. XIV, 2 and 4) are unquestionably phalli, more or less realistically modelled. (Marshall 1931 : 59).

But those pictured in Pl. XIII, 3 and Pl. XIV, 5, might not be. Marshall himself was doubtful about their exact identification. For these very much look like gamesmen, and might be so (*Ibid* : 59).

Another, a huge linga-like object was found at Mughal Ghundai (Pl. XIII, 1). The rest are said to be convention-

alized in shape, and their character was not so obvious to Marshall himself. Some of these, as Mackay notes in his description of these conical stones (*Ibid* : 476), are more or less polished at the top. This suggests, according to him, that they were constantly fingered, possibly respectedly anointed with butter or sandal paste as done today. Besides these, a *yoni* or vulva-like object was found at Periano Ghundai (Pl. XIII,7), and ringstones were found in large numbers at Mohenjodaro and Harappa. These vary in size from half an inch to nearly four feet in diameter. All the larger ones are of stone, the smaller ones of stone, faience, shell, or imitation carnelian. The most typical of these have their lower surfaces undulating. In others the lower surface is flat, and the top takes a quatrefoil form (*Ibid* : 61; Pl. XIII, 11-12).

Marshall thought and concluded after a fairly good discussion that objects in Class I were definitely phalli, and some of the ringstones, *yoni* or vulva. And these, on the analogy of the prevalence of *Linga* worship and Sakti worship to day and throughout the historical period in India, and in many countries of Western Asia, were regarded as cult objects. Hence *Linga*-worship and *yoni*-worship existed in the Indus Civilization.

Now since Marshall (1931) wrote, several sites of this civilization have been excavated. For instance, Lothal, Kalibangan and Surkotada in India, and Amri, Kot Diji, and Gumla in Pakistan, but nowhere either any of these or the Pasupati-like seal have been found.

Then the question arises, "Did these cults prevail at Harappa and Mohenjodaro only ?" or was Marshall not fully justified in reaching these far-reaching conclusions merely on the strength of a few *Linga* and *yoni*-like objects ?" We have then to ask the question, "How can we say that any of these objects, particularly the larger ones—the few realistic phalli and the large ring stones—were actually worshipped, and the smaller ones carried on the person as the Lingayats do today ? How far is this analogy, from recent ethnography, valid ?" For an

answer we should look for evidence in the archaeological record itself, that is, in the reports of the excavation at Harappa and Mohenjodaro. And much before I read Binford's *New Perspectives in Archaeology*, I had asked one of my pupils from Mysore—Shri Manjunath, to reexamine the entire data. And for this he should find out how each and every *linga*-like object was found. For the contextual evidence was indeed required. In order to prove that the larger phalli-like and the *yoni*-like objects were worshipped, these must be found in a house in such a way that there is reasonable ground for believing that these objects were worshipped. Such a detective work, spread over a year or so, showed that in no case such evidence was available or recorded, if available. On the contrary, many of the smaller *linga*-like objects were often found in drains, bathrooms and streets.

Stylistically also a very small number of objects resembled the phallus. The rest could be gamesmen, as Marshall himself had thought. Secondly, Marshall's suggestion that these small phalli-like objects were carried on one's person, rests upon a comparatively recent practice intiated by the Lingayats. It is hardly 700 years old. Hence, I concluded (unfortunately the student did not complete his thesis and left) that there was not adequate proof that *Linga*-worship prevailed in the Indus Civilization. (Sankalia : 1970).

Cones in terracotta and shell

Similarly the purpose or significance of small cones with pointed tops, usually of hard, well baked pottery, at times of shell, and in one case of bead so described in detail by Mackay (*Ibid* : 476-77, and Pl. CXXXI, 47-55, Pl. CXXIV, 8-20) could be probably understood, if a careful watch is kept while digging in the Intermediate and Late Level houses of Mohenjodaro. (Fig. 15) For such cones do not seem to have been found elsewhere in other Indus Valley sites. At the same time, these should be looked for at Kalibangan and Lothal. If not found now, then small objects provide an interesting variability in the culture complex among sites of the Indus Civilization.

Sling balls

As opposed to these we may cite an example from the same work of careful correlation of the object described and its surroundings, so that we are reasonably sure of the purpose for which such objects were made, and from this sure knowledge we can lay down a general law or principle that wherever such objects were found in the Indus civilization, these might or should have served a similar purpose.

The objects, under discussion, are sling balls. These are of two types : round, about the size of a large marble, and other ovoid in shape and the form averaging 2. 5 inches long and 1.6 inches in diameter. (Marshall Vol. III Pl. CXXX, 4, 5). Both types were made by hand with degrees of finish, and baked in all cases.

The round pellets may have been propelled by a sling of ordinary type or by means of a bow still used in Sind for killing birds.

Weapons of offence and defence

The ovoid or oviform baked pellets must have been used for a similar purpose; fifty or more of the egg-shaped lumps averaging 4 inches long and 2.5 inches in diameter were found carefully stored in a large pottery vessel. *Further south in the same area a number of* large pottery balls were found in confusion upon the ground *outside a very thick enclosure wall* (Italics mine). Hence the conclusion that all these should be regarded as weapons of offence or rather of defence. (Mackay : 466).

Test the hypothesis

However, as new archaeologists, we should try to test this hypothesis. Mackay had already posed the question. "Why was the shape ovoid or oviform ? Did this shape impart a spin to the projectile and made it more accurate in its flight ? Would such a form lead to a certain amount of ricochet and thus increase the chances of a hit ? Or was this form simply copied from nature, where the river pebbles are more commonly

ovoid in shape than round?" (*Ibid*).

Now these questions are quite relevant and important if we want to have some positive knowledge of warfare in prehistoric times. Experiments in their use should have been conducted when the objects were first found or when similar ovoid baked clay pellets were found in the renewed excavations at Mohenjodaro by Wheeler. Deposits of these and of larger clay missiles weighing 12 ozs each had been assembled as "ammunition stores" behind the brick parapet between two towers as the southeast corner of the Mohenjodaro citadel. (Wheler 1966 : 15).

Pending such experiments, we might conclude that these round or ovoid baked clay pellets were used in warfare, and wherever—in an ancient or prehistoric settlement—these are found in large numbers these areas would have served as a defence outpost.

Kalibangan

Some of the concepts, methods and techniques newly introduced or emphasized by archaeologists at present should or could be profitably applied while working on the report or study of Kalibangan.

First, for instance, is the concept or view that Harappan settlement had an acropolis or fortified habitation at a higher level, and a Lower Town or a habitation on the lower level. In the former the ruler or priests or some such people lived; the masses, citizens or townsmen as such, lived in the other settlement. Now this distinction is primarily based on the architectural peculiarities noticed first at Harappa and then at Mohenjodaro, Lothal and particularly at Kalibangan. At the last mentioned site, "this entire settlement" is on mud platforms and the whole surrounded by a mud brick wall.

Now we may well ask, "This is alright as far as the differences in architectural features are concerned, but how was the life of the two sets or types of residents different? If it was so, how shall we know it?"

For an answer or answers to these questions we have to look into the archaeological record—small and big finds or antiquities. We may ask for instance, "did the priests or rulers use chert blades ?" If they did, were they also made in the citadel, as they were in the Lower Town as shown by Mohenjodaro ? In fact, this question could be easily answered if the various finds of chert blades at Lothal and Kalibangan are plotted, as done by me for Navdatoli. Likewsie, we may examine about the types of pottery vessels, seals, beads and numerous other things. What is needed is an inquiring or inquisitive mind, a desire to pose and answer questions. Otherwise we shall have the same old type of reports about chert blades, pottery, etc.

The Place of Painted Pottery in the Indus Civilization

One more question before we leave the Indus Civilization. It is well known that unlike Navdatoli where painted pottery was nearly 90% and formed the bulk of the pottery in every household, in the Indus Civilization, as known from Mohenjodaro, Lothal and Kalibangan, painted pottery formed only a small percentage. Then the question arises, where were the few painted pots kept, in the kitchen, or in other rooms, and what was their function ? Of course, an answer to this question can be had only if a careful watch is kept when a site of the Indus Civilization is being excavated. Failing such observation, our knowledge about the function and place of the painted pottery in this civilization will remain vague.

Osteoarchaeology and Sociological inferences

At Kalibangan, Sharma (1969–70 : 109–113) while studying the human skeletons from an osteoarchaeological point of view found interesting evidence of

(i) a surgical operation (trephining) on a child's skull because it had accumulated too much water in its cells (hydrocephalis),

(ii) congenital perforation in the bone and exostosis,

(iii) a deep wound–cut marks–on the inner side of the knee,
(iv) a person who was born a cripple (but was looked after and buried ceremoniously—a clear evidence of human sympathy from the family and society),
(v) and an individual whose first and second right molars showed marked attrition of their cusps.

He attributes two causes for this dental attrition : (i) the habit of using left (or right) side for chewing, and (ii) coarse food mixed with powdered stone grains.

But he also tells us that the teeth of other Kalibangan human skeletons (including old people) studied by him were very good, because "they were at least not vigorous flesh eaters." (I am told that the view is now otherwise.)

Now from this variance in teeth behaviour one might draw important sociological inferences. If the teeth of the Kalibangans were good, and not worn out, they might belong to a fairly rich vegetarian group; secondly the grain, although ground on sandstone querns must have been properly sieved after grinding, whereas the person whose molars on the right hand side were very much worn out must have belonged to a poor class or group, where any food that was available was eaten (or who had no family, and facilities for sieving, etc., were not there). However, all these inferences should be further confirmed by the archaeological record, to some extent, at least. Thus, the grave of this individual might not contain many pots and pans, if he was really poor, and if he belonged to a working class, a labourer, his house or grave might lie among such a locality or group. Now while excavating or even after excavations are over, such inquiry has to be made, and even if partially confirmed, our sociological inferences will have some significance. Otherwiee, it will remain an osteoarchaeological inference by a physical anthropologist !

Langhnaj

From this we shall pass on to our own excavations at Langhnaj. Here during our 2nd and 3rd seasons of excavations

we found a large shoulder blade and a long rib of an animal. This and all the large bone finds were three-dimensionally recorded, and photographed. We could not immediately identify this large animal. Later the remains were identified as those of rhinoceros. This identification itself has led to important inferences about the climatic conditions in Northern Gujarat some 4000 years ago. This knowledge was certainly welcome, as it was not available from any literary sources.

But had we been able to identify the animal then, in 1944–45, and posed the question whether the rhinoceros remains represented one animal or two and whether these were brought to the site, that is up the mound, and then butchered, we could have gained an insight into the bioarchaeology, that is about the life of the people much more than we do today.

Of course, it might have been necessary to widen the trenches, and if by this means, we could have collected almost *all the rhinoceros* bones, then we could have said from the area within which these were found, whether the site was used for butchering the animal on the spot, and if so, we could have estimated (of course roughly) how many persons it could have supported. However, this subject was in its infancy then, and naturally we were anxious to find and collect only important large bones. Now, if Langhnaj or any site in North Gujarat is redug, these and other methods have got to be adopted.

Cow-slaughter—since when stopped ?

In fact, this scrupulous collection of all animal remains is absolultely essential in all excavations, whether these be historic, protohistoric or prehistoric. In a historic site like Kausambi, we should like to know if the cow was eaten, and if so since when ? Secondly, whether beef was eaten by only a section of the population, or by one and all. Now answers to all these questions can be had if in any future excavations at Kausambi, Mathura or Ayodhya, if the excavators keep in mind these questions, and sample the sites accordingly, collect *all* the animal

bones and get them examined immediately (before they are lost or thrown away).

Settlement pattern—Painted Grey Ware Period

Answer to some larger questions in historic and proto-historic archaeology can be likewise tackled, for instance, the settlement pattern during the Painted Grey Ware Period. Since its first discovery at Ahichchhatra and then at Hastinapur in 1951-52, literally hundreds of sites are being reported from Northern Rajasthan, E. Panjab, Harayana, Uttar Pradesh and Bihar. Distribution map of an ordinary type would show or confirm the wide extent of this culture. But this is not enough. If we plot these sites taluka-wise and taking into consideration their proximity or otherwise to sources of water—river or stream—trade routes, market places, and sites of political and religious importance today, and all through historical periods, then we would know how these sites have developed, or have been deserted in later times. Wherever possible, the extent of the P.G.W. occupation could be noted. Of course, all this cannot be done in a day or by a single agency. But if the universities in Rajasthan, and Northern India in general work in cooperation and chalk out a plan, taking a taluka or district as the unit, then some such work is foreseeable not beyond accomplishment within a foreseeable future.

30 Walled towns of the Satavahanas

In historical archaeology, problems such as the existence or otherwise of 30 walled towns of the Andhras or Satavahanas as mentioned by Pliny can be systematically tackled.

Until the discovery of Kolhapur in 1945-46, and then Chandravalli in 1947, Nasik in 1950-51, Nevasa in 1954, we were not sure of the existence of towns or cities, as defined by the presence of large brick-built buildings, with wells, and items of foreign trade and foreign and indigenous coins in their debris. Now with these sure clues we should plot all the places

mentioned in indigenous literature—epigraphical and otherwise—and mentioned by Graeco-Roman geographers and travellers, also those discovered through archaeology, and those lying on the ancient trade routes. Such a study would surely yield a number larger than that mentioned by Pliny (which itself, by the way, is not much or fantastic), then a few of these towns, particularly those where the debris of this period are not hidden by later historic deposits should be selected for a ground survey, aerial survey and a survey by electrical resistivity method. Sites like Ter, Kondapur, Chandravalli are sure to give indications of a walled town, but these are difficult, if not impossible to locate in sites like Nevasa and Kolhapur where the Satavahana habitations lie under a 20 ft. deposit of later debris. With some planning, an understanding of this long standing problem can be had.

Problems in Palaeolithic Archaeology

Here there are various problems. As discoveries are reported, the general Stone Age map of India is being gradually filled up. Now one may say that the Early and the Middle Palaeolithic man had lived practically all over India from Kashmir to Kanyakumari and Dwarka to Bhuvaneshwar.

The same wide distribution we cannot yet claim for the Upper Palaeolithic. So far the best evidence comes from the Kurnool and Chittoor Districts of Andhra Pradesh, whereas stratified evidence is being obtained in Madhya Pradesh and Maharashtra. It is probable that similar evidence will be found in Bihar, Orissa, Karnataka, Gujarat, Rajasthan and U.P.

However, these distribution maps are of a "coarse-grained variety." These make no distinction between sites, not only their size, but their relation to the topographical and environmental features of the area.

The reasons for this are inherent in any research project. Initially the things are to be found, then these are roughly identified and classified. Then follows refinement. This needs planned strategy, expertise and enough resources. For instance,

the collection of tools—whether these be of Early, Middle or Late Palaeolithic—should be from a stratified site, preferably from a camp or factory site. If such a collection is made, we may know the following :

(1) the preferences of Early Man for this or that location–river bank, lakeside, rock-shelter,
(2) the various activities he engaged in and their specific nature,
(3) the probable function of tools and,
(4) their relation to the raw material,
(5) the types and ages of animals hunted (provided animal remains are found),
(6) the likely source of other food supply by studying the present flora in the region/locality,
(7) something about the social organization.

So far only eight sites have been excavated : (1) Adamgarh, (2) Lalitpur, (3) Mahadeo–Piparia, (4) a site near Navdatoli, (5) Bhimbetka, (6) Chirki–Nevasa, (7) a site near Patne, District Jalgaon, (8) a site near Hunsgi, District Gulbarga. The first five are in Madhya Pradesh, the next two in Maharashtra, and the 8th in Karnataka.

Only Lalitpur, Bhimbetka, Chirki-Nevasa and Hunsgi have been horizontally excavated, so we have contextual evidence, viz., the relation of tools *inter se*, and their relationship to any architectural or other feature in the site. Adamgarh and the two other sites on the Narmada provided excellent stratigraphical evidence about the type of Early Stone Age tools. Unfortunately none of the horizontally excavated sites yielded anything besides a large number of tools—handaxes, cleavers, thick one-edged choppers, in mint condition, but the debitage which should normally accompany or be associated with these assemblages was also not there or if the debitage had remained, it did not provide any fits. Hence we could not conclusively say that each was a factory or a workshop. In the absence of any definite house remains—such as stones

arranged for making or supporting a stone or mud wall, it was also not possible to declare the site as a habitation site. Still the large number of tools definitely indicated that the Early Man had sat there for days and months or years !

In the absence of such contextual evidence what could one do except study the tools by the traditional as well as the current analytical technique ?

Traditionally the vast Chirki-Nevasa collection spread over three seasons and now numbering over 2000 tools would fall into

(a) highly pointed handaxes—all or mostly of Upper Acheulian type,
(b) cleavers of all types, made largely on side flake technique,
(c) thick one-edged choppers or knives,
(d) choppers, comparatively few, and
(e) other non-descript tools.

The material is largely fine-grained trap, from the nearby dyke, but occasionally even the coarse, easily available reddish basalt has also been used.

The site just overlooked the Pravara river, and the man encamped under or near the coarse grained basaltic hillock.

No animal bones were found, except an elephant tusk, but this too was not from the excavated site. Dr. Gudrun Corvinus who first excavated the site, has now practically measured *all* the 2000 and more tools for attribute cluster analysis.

When this study is published we shall know how far this so-called factual or unbiased typological classification following the latest scientific technique corresponds with the traditional classification.

However, in the absence of any other data, such as Lewis Binford had for the Mousterian in France and Sally Binford had for the Emiran point in the Near East, in the form of different location of tools, or indicating (probably) occupation by different tribal or class groups under different climatic conditions, or evidence indicating different climate with the

help of the flora, fauna and palynological studies, how shall we interpret the change/changes in tool types including difference in the raw material ? Such differences or variations are markedly seen in the Chittor District and also in the excavations at Sangankal, and in the latest excavations at the cave and rock-shelters called Bhimbetka, District Raisen, Madhya Pradesh.

In fact, these very few, careful stratified excavations confirm the impression formed by the observation and study of vast collections from many parts of India. And the mind working as a computer, as Professor Bordes said in our discussion at Montreal, Canada, could not only sort out the basic types, but even arrange the collections, whether large or small, in broad chronological groups.

We should all seek meaningful refinement in our typological studies, but there should be scope, an opportunity for achieving these. Unfortunately, some scholars, who had the opportunity to do some field work, but did not utilize this opportunity and who have not faced the problems cited above, have been running down the work hitherto done. (Malik 1968).

Briefly then in the Indian Palaeolithic the main problems are to discover the camp/factory sites, excavate them carefully, and derive whatever information one can get about the life led by Early Man and his successors. With the help of this positive data, we may also refine our distribution maps. However one other point might be emphasized.

Distribution maps

Once the main discovery of Stone Age sites was made in Gujarat, Maharashtra, Karnatak, Madhya Pradesh and Rajasthan, many other discoveries were made often by untrained workers. These help swell the categories of sites, but are not always useful in preparing a proper distribution map. Hence the need of scholars trained in geology, geomorphology as well as prehistory for preparing a meaningful distribution map.

Before leaving this section, one might raise the funda-

mental question : "What or how many different kinds of activities would you credit or expect the Early Man to perform, and how many could you possibly infer from the traditional, typological studies or the current attribute cluster analysis ?" This question would be relevant in India where there are not many marked climatic changes between different regions, and even the seasonal changes are not such that these changes would be reflected in the tool typology and Factor Analysis. Of course one could certainly seek an explanation for the occurrence of a large number of comparatively thin, long, pointed handaxes at Chirki Nevasa, or of cleavers at Lalitpur, and at Bhimbetka or at Hunsgi. But for this understanding would it be necessary to measure all the tools, or would it be advisable to carry out actual experiments in the use of a few distinct tool types ? My experiments have shown that the cleaver is indeed useless for cutting dry trees, whereas the handaxe, particularly the pointed variety, would be useful in digging and removing grubers, etc. But the question still remains, "why so many varieties of pointed handaxes, many showing gradual improvement in form and technique" ? Again, this improvement is not confined to one area, or one country, but seen everywhere within fairly comparable time ranges. Hence the traditional typology has some value, though it can be refined with the help of new scientific methods.

Middle Palaeolithic

Similar are the problems and their solutions in the Middle Palaeolithic. So far, during the last 20 years, only broad categories of tool types such as scrapers, borers/awls have been recognized. We do not indeed know their exact function. Once these were regarded as primary tools; now it is held that these were really meant for manufacturing larger tools/weapons of wood and bone. The latter have not been found any where; at least not in Africa and India. Unless, therefore, a camp site of this period is found, and the tools located in their contextual position, we shall certainly remain ignorant of their

real function, and hence of the life or activities of the man of this period, though it is generally assumed that hunting and food-collection was the main job. Meanwhile the extant collections can be studied only typologically by the traditional as well the scientific methods.

The Upper Palaeolithic

In the Upper Palaeolithic, the primary problem is to extend the present very limited distribution to other parts of India. A glimpse has been given by Central Gujarat, if the available finds have been correctly identified. Rajasthan might follow suit, as well as Kutch and Saurashtra. This is not wishful thinking but based on the experience gained so far. The question now is, "How shall we interpret this great change in the tool type, first from the Early Palaeolithic handaxe-cleaver choppers to the small scraper-borers on fine grained material and then from the latter to the fairly thick blades and burins on the greenish fine grained quartzite in Chittoor and Kurnool and thick chalcedony/jasper blades and burins elsewhere" ?

Mind you, we have no other data—climatic, faunal or floral or locational—as in the Near East, or in Europe and Africa. So far the same sites have produced evidence of three Stone Age industries. These can be separated only stratigraphically and typologically. I have, following the traditional line of thinking, postulated new or fresh cultural influences, as there is no reason for a change in the behaviour of the people inhabiting the sites previously. (Sankalia 1973, 1974).

Mesolithic

Our records for the Mesolithic are not much better, though now we have reliable stratigraphic evidence from Langhnaj, Adamgarh, Bagor and Sangankal. For the first time we have, besides numerous microliths, abundant animal remains, and no less than 14 human skeletons from Langhnaj.

The picture of the life at Langhnaj can certainly be ampli-

fied, if now excavations are carried out horizontally. We might find evidence for erecting any wind screen in the form of stone pebbles of quartzite, and above all find out how many animals of each species—rhinoceros, nilgai, pig., etc., were killed. Had we been able to identify the rhino bones in 1944 *at the site itself*, we might have been able to say whether only one rhino was butchered or many, whether it was young, adult or old, and by ascertaining the protein content from a study of the bones, might have been able to calculate the amount of subsistence available, and from this inferred the probable number of people living on the mound (preferably around the area dug by us).

The Langhnaj lithic industry had been classified on traditional lines into types. This may now be reclassified by the modern techniques and the results compared with the old.

For Bagor, similar work is being done by Dr. Misra. At Sangankal again the difference in the raw material and the three/four categories of tool types is clear (Sankalia 1969). It is again supported by the climatic variation inferred from the buried soil horizon. What is now needed is to extend this limited evidence from a small excavation from one site alone on a much larger canvas. This is not difficult if one explores other parts of Bellary, and the adjoining districts, particularly bearing in mind the juxtaposition of the site overlooking a perennial stream at the foothill of a granite hill bearing neolithic habitation. Whatever might be said for the traditional point of view, the difference in the raw material and the tool types is indeed significant at Sangankal as well as at sites in the Chittoor District.

Neolithic-Chalcolithic

With this cultural phase we enter a more civilized stage. Already our excavations at Sangankal and Tekkalkota, Navdatoli and Inamgaon have given us some idea of the type of houses in which the man of this period lived. These were essentially one room, round, square or rectangular houses, with

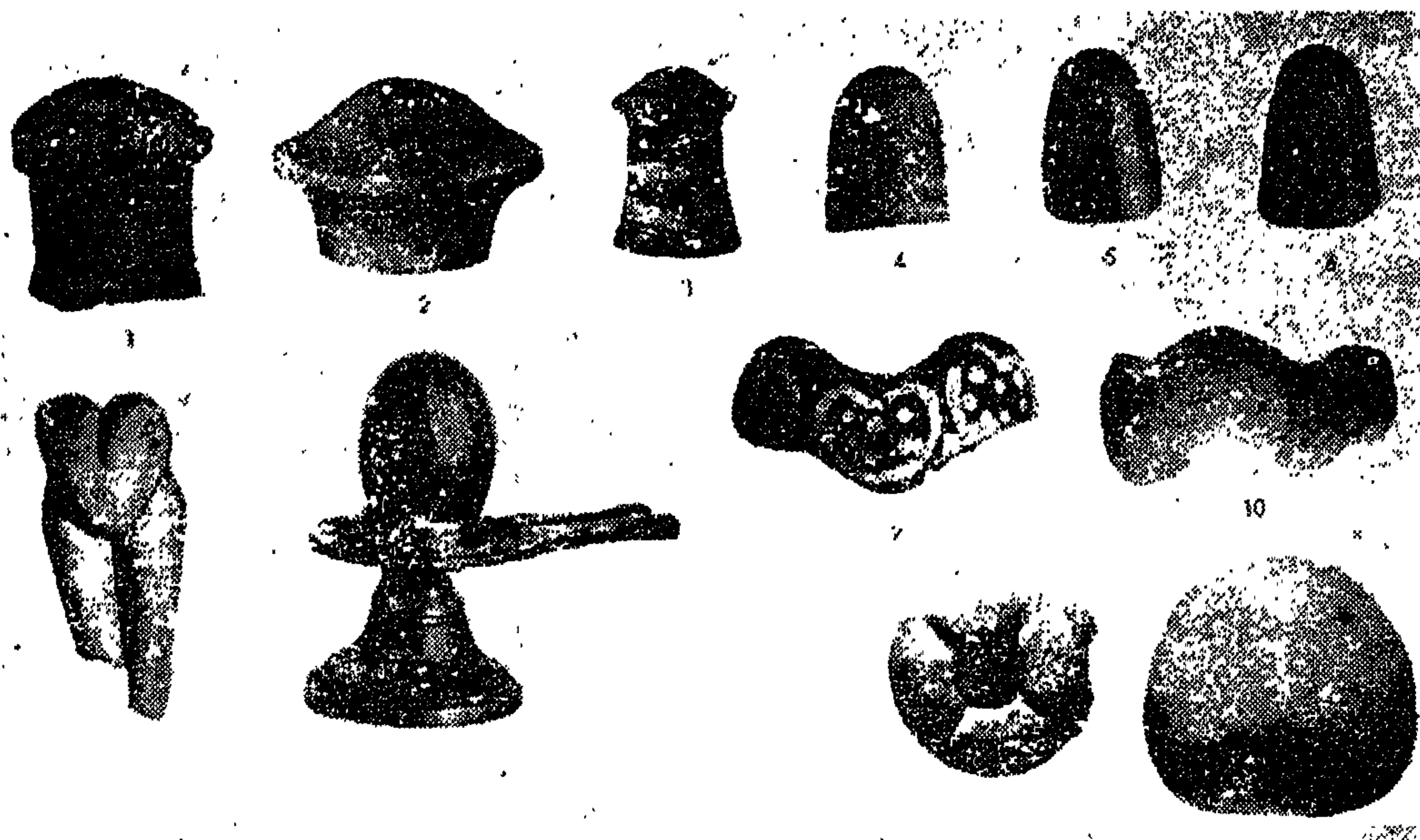


Plate 7. Small *Linga*-like and *Yoni*-like ringstones from Harappa and Mohenjodaro

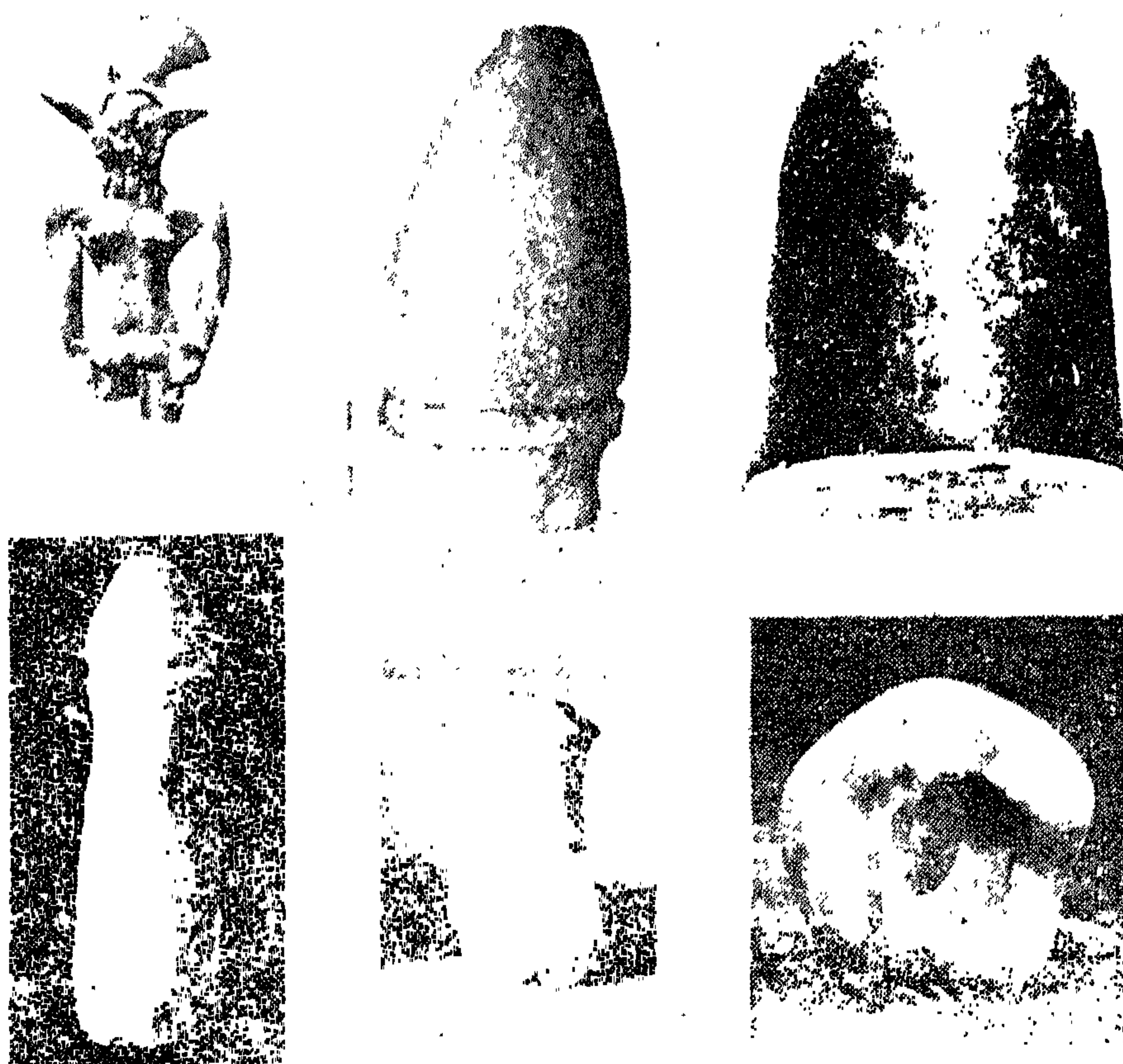


Plate 8. *Linga*-like objects and *Yoni*-like ringstones from Harappa and Mohenjodaro



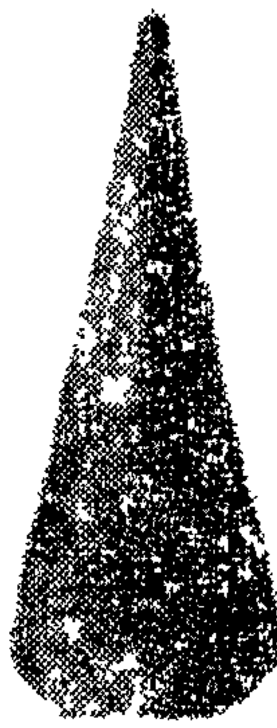




Plate 9. Terracotta cones from Harappa and Mohenjodaro



Plate 10. Mandible of a human skeleton from Grave 12, Kalibangan, Rajasthan. The right molars and premolars of the mandible show marked attrition in comparison to their left counterparts

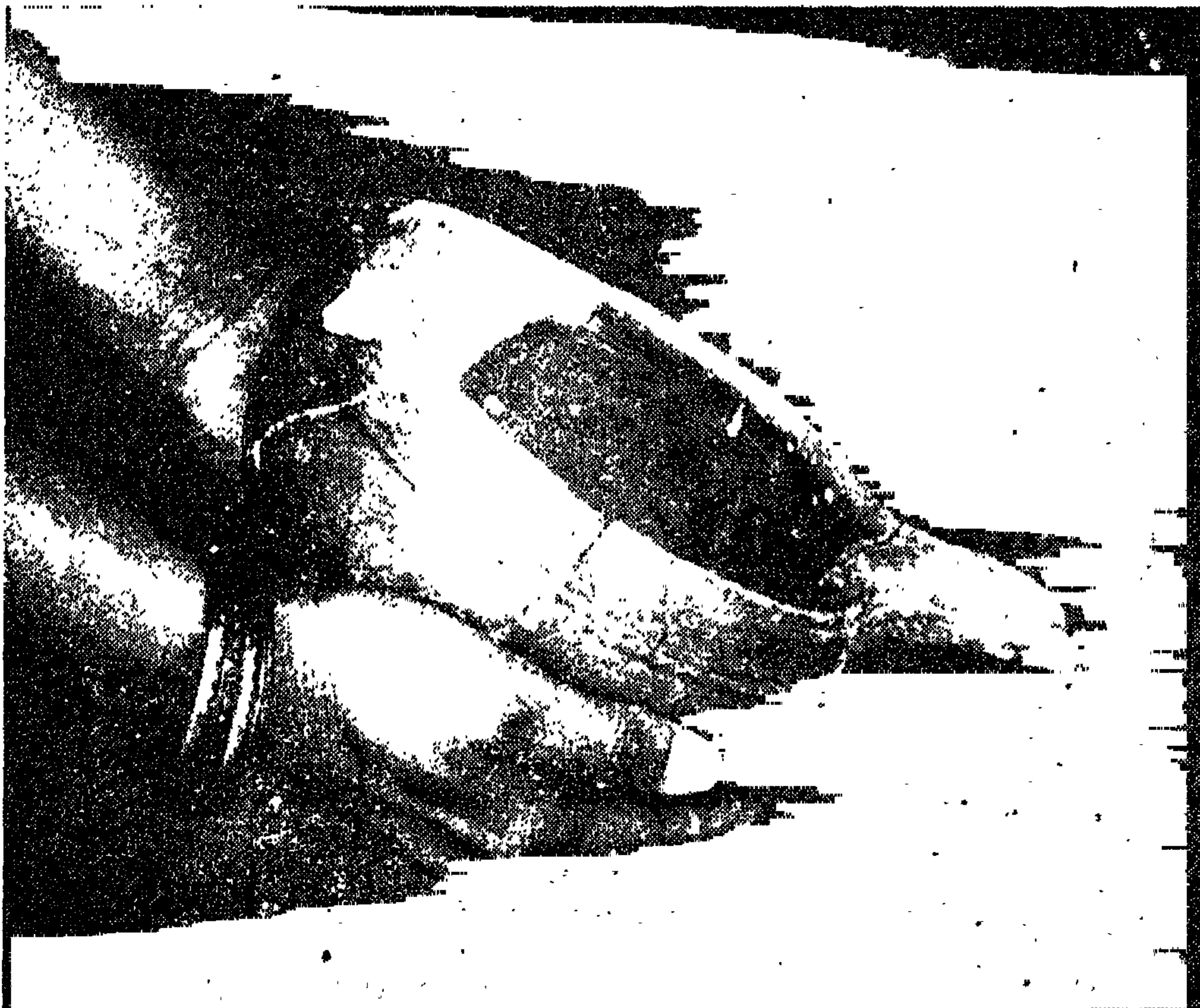


Plate 11. Photograph showing the probable use of the channel-spouted bowl from Navdatoli, M.P., c. 1300 B. C.

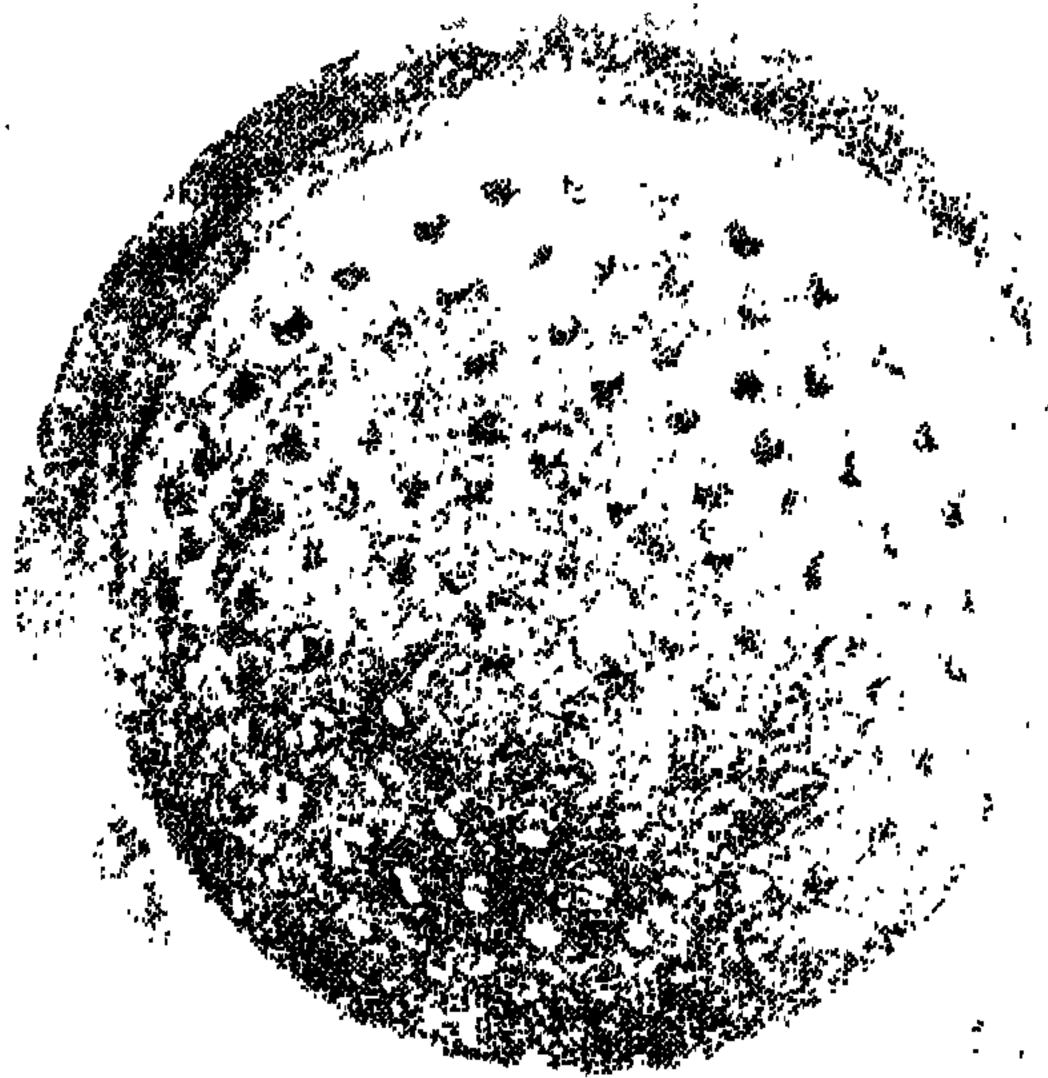




Plate 12. Top and profile of a modern perforated bowl (pottery) from Andhra Pradesh

[p. 94

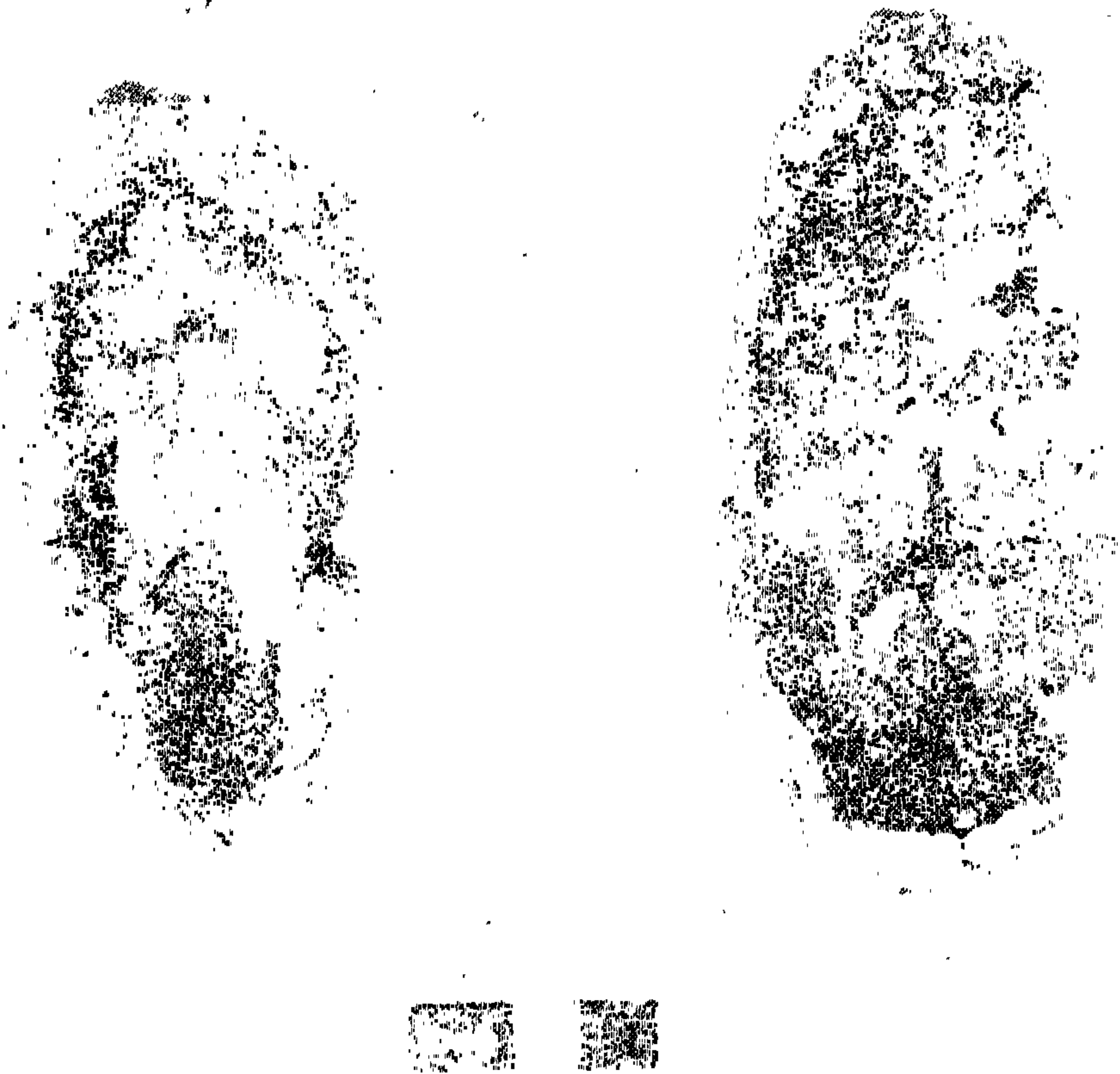


Plate 13. The container and cover of a clay box containing a sleeping female figure (goddess) from Inamgaon, Maharashtra, c.1300 B. C.

[p. 95

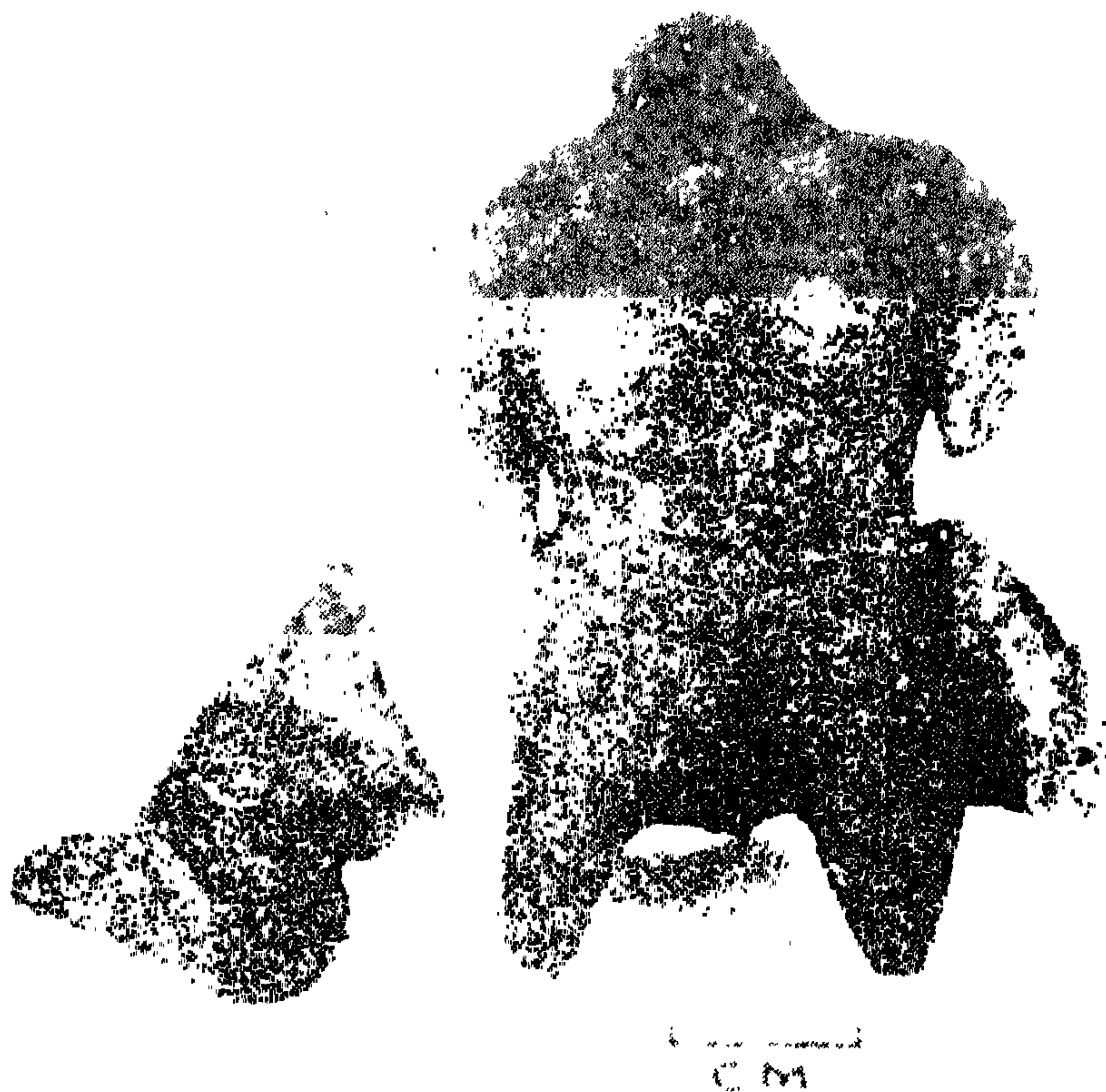


Plate 14. Clay female figure (geddess) standing with the support of a circular stand kept in the clay box Inamgaon.

[p. 96



Plate 15. Headless clay figure (goddess) standing on a bull, Inamgaon

all the essential "furniture" a small pastoralist-cum-farmer should have a fire-place (*chulah*) made with three stones and a clay-border or a square clay bordered pit, a storage jar standing on three terracotta legs or stone slabs, and a few pots and pans, ground/polished stone tools and chalcedony/jasper chert blades for the everyday needs in the home, viz,for cutting, scraping and boring. As our evidence shows, from all the sites, these blades were made in every home. It was a cottage industry. The dead were buried in the house in Karnatak and W. Maharashtra, but we do not know what they did at Navdatoli. Animal food probably formed a substantial part of the subsistence at Tekkalkota and Sangankal, whereas at Inamgaon and Navdatoli, wheat, barley, lentil and other grains were available. We have therefore to assume that the Chalcolithic people in Maharashtra and Madhya Pradesh were culturally more advanced, as they certainly were because of the preponderance of fine baked pottery they had, and relied almost exclusively upon the lithic blade tools for their activities in the home and the field.

At Inamgaon we have gone much further in our methods of excavation and data collection. Not only the blade, etc., from each house, but even the sherds, let alone intact pots and pans, are kept separately, and so also bones of animals.

Hence besides yielding knowledge about the socio-economic conditions from the space-time study of blades, we shall have some idea of the family unit as represented by pots and pans in each excavated house. Further we might also infer how much animal food this family consumed, when the bones from each house are not only identified, but their food value estimated (by taking into consideration the parts of the animal body they represent). Needless to say that such studies are possible only if a trained zoologist is available on the field.

With regard to the large number of stone balls found in the topmost houses of Period III (Late Jorwe phase), it occurred to us that, assuming that these are sling balls, we might know their efficacy in hunting and warfare if their range is determined by their actual use. We therefore managed to procure a sling,

but so far are unable to find a person who can hit accurately at a given target !

Another problem has been troubling us : "Would the Chalcolithic people, wherever these balls are found and identified as such, expend so much time and labour on an object which if once used is likely to be lost ?" Of course, this question might be parried that this is a sign of advancing cultural development. We spend lakhs of rupees in manufacturing a warplane, which is most likely to be destroyed. Thus this question is not much relevant, once their efficacy in warfare is established. For man can afford to be extravagant.

Other Furniture

The significance of other "furniture"—saddle querns, crushing or grinding stones, and platforms (for erecting, wicker baskets), storage jars on flat stones or raised legs, and other pots and pans, if found in their places, as sometimes—though very rarely—in a house at Navdatoli, and in a house at Inamgaon, as well as fire places (*chulahs*)—has been invariably interpreted ethnographically and historically. It might be said without contradiction that the daily life–activities–in the chalcolithic hut was not fundamentally different than in the villages today, though the material of which the objects—pots and pans—are made might differ.

Now the problem is : "Can we say anything more than this ? Say about the social organization, about the relation between the residents of the few houses in Period II or the 38 houses in Period III, about pottery making, whether for instance, the design on pottery were painted by women (as is the common assumption in America) and the beads and blades manufactured by men" ?

Yes, we could if we have the kind of data that the Americans have found in Arizona. Until then, as long as our excavations remain small and only of a vertical nature, scholars will continue to play with pottery and palaeoliths. For making models and hypotheses and seek explanations, some reliable

evidence about the human and animal lifeways should be available.

The question is : "Is or can archaeology be so exact, accurate, precise, as to yield all the lost data, knowing as we do in what conditions or circumstances we find/collect our data ?"

New let us review once again how we excavate, collect and record data. The technique of excavation would vary to some extent depending upon the medium in which we are working. These are as far as I know :

1. sandy mounds, containing the relics of a Mesolithic way of life—microliths, animal bones, perhaps pottery, buried human skeletons and sometimes rows of stone for mudwalled houses;
2. earthen mounds, containing the ruins of mud-walled houses with stone plinths.

Digging

The mounds are fairly large and the earth/sand so hard that labourers have to be employed and actural digging done with large pickaxes, and the earth removed. This is certainly a much better thing/technique than to remove the debris with the help of a bulldozer, or with the blade of a tractor. We cannot work with a small pickaxe, shovel and knife at the outset. These are of course used when we are *about* to reach the actual habitational (or till) debris, and for noting their arrival a trained supervisor is always there. He himself takes the work of digging, cleaning and exposing the object, whether it be a pot, skeleton or an architectural feature.

The digging is again stratigraphical following the natural earth layers, and whatever objects are found are recorded three-dimensionally in relation to these layers. However, we must remember, and this fact cannot be adequately stressed that all these recording of objects, indicating the spatial and even vertical distribution, as far the small finds—such as microliths, beads, stone axes, hammer stones and potsherds—is concerned, is at best *approximate*. For, as Lewis Binford wants, and laid down as a condition for drawing meaningful (or farfetched) conclusions,

the archaeological record must be undisturbed, and has been investigaged in a manner which takes sampling error into account, and that order in the record derives from the systematic relationships, which characterized the social life of the cultural unit under study. (Binford and Binford 1970 : 85).

Even if the latest scientific methods–such as the electrical resistivity method and the X–ray camera–are employed in ascertaining what the contents of the mound are, we cannot determine the exact spatial distribution of such finds, for these almost inextricably become one in the matrix of limey sand, as at Langhnaj, (or the hundreds of mounds in Northern Gujarat and even Rajasthan), or the earthy matrix at Navdatoli, Inamgaon and innumerable other sites. We cannot penetrate through this dense medium, and it will be of no avail and much less practicable if we were to dig *ourselves* as they do in Europe and England. No doubt, the relation of these small finds is noted whenever they occur together with a skeleton, or as intact pottery. At Nevasa we were lucky to locate the first find of a fully Polished Stone Axe which was placed on a huge storage jar and in another excavated square the remains or traces of a small "workshop" for manufacturing stone axes—an incomplete axe, a stone anvil, a hammerstone or fabricator and one or two chips. (Sankalia *et al* 1960). But by and large, in spite of the greatest care and skill, the position of the small finds has to be regarded as approximate. This I have acknowledged openly before, while writing on the "socio-economic significance of the lithic blade industry from Navdatoli." (Sankalia 1967).

Inamgaon and Surkotada

The same qualification would be true of our observations of the number of pots and pans, as well as animal bones in our current excavations at Inamgaon, or of the occurrence of certain tool types in the cave/rock–shelter excavations at Bhimbetka. The things could be a little or considerably better in the excavation of Harappan sites. For instance, in the recent excavations at Kalibangan or Surkotada, the excavators

could have noted the spatial distribution of different types of pottery fabrics, and several other objects. For in these the houses are built of burnt or mud bricks, or local stones, and hence the boundaries of the excavated area can be easily determined. And once having "entered" the house, some more meaningful information might be gleaned if the pottery and other small objects are studied house- or area-wise. But this study has to be done almost every day, or at least once a week, by the supervisor himself, recording and studying the finds. Unless there is a continuous "feedback", to use an Americanism, that is the study of the finds as they are found and the number of small and large questions raised by them, the finds will continue to be studied as they are today.

Even at the risk of being misunderstood, I might say that what the Americans are preaching, viz., asking questions and the space-time study of finds, is being done since 1944, otherwise we could not have produced the vertical and horizontal distribution of microliths, bones, etc., at Langhnaj (Sankalia 1965), at Kolhapur (Sankalia and Dikshit 1952) and then at Navdatoli.

Tekkalkota

At Tekkalkota, there was an excellent opportunity to test the significance of the occupation by the Neolithic inhabitants of the various "terraces." Almost everyday, while going up and down the hill and noting a feature here and a feature there, I asked myself for its explanation, and then passed on the same questions to my coleagues. Of the several such questions, I would cite only two problems here.

On the Terrace I at Tekkalkota, there was one completely detached, circular, cluster of stones, overlooking the valley. I thought that this isolated habitation which we called "honeymoon house" could have been either (i) that of the headman of the people/group staying on Terrace I, or (ii) a watchman, for observing the movements of the people in the valley below. Now these models or hypotheses have to be tested by excavating the settlement, and finding such evidence as would

prove the truth, even partially. Careful excavation enabled us to say that these were two clear periods of occupation, and where possibly the entrance to the circular house was. We also tried to determine the height of the mud-walls by taking into consideration the thickness of the mud/clay debris. (Sankalia 1964; Nagaraja Rao and Malhotra 1965). However, no specific evidence was found—except a teapot like pot in a crushed condition on the skeleton of a child, buried in a narrow cleft between the boulders which could enable us to say that the inhabitants of this isolated "house" were different economically or in social status from others living on Terrace I.

In another house on the large open terrace we found a stone axe placed under the raised legs of a large storage jar. This led me to say, "Here was a cupboard of the Neolithic people, with open space under it to keep the household objects, as we do today in a small flat in a city !"

We were also surprised to find that very irregular, bouldery places/corners on the terrace had been occupied by man. For here were found large number of blades, cores, and a few potsherds also. This inference was further confirmed when at two or three such places we also found traces of the foundation of mud walls in the form of a row of stones and natural boulders. Briefly, whatever more knowledge about the lifeways of the Neolithic inhabitants of Tekkalkota we could extract from all these observations was extracted.

Ethnography was not also neglected. At the foot of the Tekkalkota Hill lives a nonliterate tribe called Boya. They still follow a hunting-cum-pastoral profession, though the former has now almost become a ritual with the dwindling game. These people live in round huts, whose borders are outlined by small and large boulders. These support a flimsy half-mud-cum-thatched wall with a conical, thatched roof. Inside, small pots and pans are often (or when not in use) hung on the ceiling of the roof, while outside often lie a huge saddle quern on which spices and rice are ground. Between two or three houses there is a huge storage bin standing on terracotta legs and covered with rags. Grain is taken out

from the front centre of such a bin, as and when required. Our colleague, Dr. K. C. Malhotra, carried out a sample anthropological survey of the physical types exhibited by these people. (Malhotra 1965).

This study revealed that though there might be some correspondence between the present day Boya and the prehistoric settlers of this region, still it would be wrong to say that the Boya are the descendents of the latter, or in anyway connected with them. In some respects, as far as their pots and pans are concerend, they seem to be far less advanced than the Neolithic people. Of the latter's distinctive stone tool, viz., the ground axe, they have only a faint memory.

Ahar

Our experience was similar at Ahar, a site in S. E. Rajasthan, adjoining Udaipur. Here the Bhil live in houses which remind you of the ground-plan of the houses built on stone plinths on the adjoining mound, 4000 years ago. Still in other lifeways there appears to be a marked difference. (Nagar 1966).

Ethnographic parallels : Dangers

Ethnographic parallel has its dangers as well. We cannot always form a correct behavioural estimate by observing and recording a house left almost intact by the resident only a few years ago. This has been vividly illustrated by Bonnischen (1973 See Appendix). Still in a country like India where the most ancient past lives with the most recent present, one can and should always try to learn whatever one can by looking around, and also by making a special effort to study the lifeways of the people at various levels. Hence at the Deccan College we have now established a Chair of Ethno-archaeology.

In this connection a very interesting and instructive information was reported to me by Dr. Suman Pandya, one of my former pupils. In the year 1973, Northern Gujarat suffered from one of the most severe floods in the Sabarmati. To study the after-effects of this flood, Suman was sent out by

Gujarat Vidyapeeth, Ahmedabad. She noticed that those who live in the lower reaches of the Sabarmati, around Dholka, Lothal and other sites near the Gulf of Kutch, use small boats for their movements when they are marooned, and also throw up protective walls of nothing but mud around their villages/houses. This information again leads me to revive the old surmise I had made about the use of mud platforms on the sites of the Indus civilization, against Wheeler's criticism, where brick wallings are made with rectangular bastion. (Wheeler 1966 : 30).

I may mention that the conception of a contextual study of objects in archaeology is not quite new (though it might be new in America). Even in historic archaeology, we should not study, say, sculptures or temples in isolation.

Historic Archaeology

What the New Archaeologist wants was done nearly 40 years ago. All the data from various 10th-14th century temples in Gujarat (including Saurashtra) was tabulated, and analysed. And then I wrote :

> The result of our analysis of shrines dedicated to different gods and goddesses shows that :
>
> (a) Temples of
>
> (i) Siva generally face the east or west;
>
> (ii) Visnu even south;
>
> (iii) Brahma east or north;
>
> (iv) Surya usually east;
>
> (v) A *devi* north or east;
>
> (b) Ganapati is in the centre of the door-lintel generally in all shrines.
>
> (c) (i) A purely Saiva shrine has Saive *parivaradevtas*;
>
> (ii) An ordinary Saiva shrine has other deities as well;
>
> (iii) A purely Vaisnava shrine has Vaisnava gods and goddesses;
>
> (iv) A shrine of Surya has Surya figures in niches besides those of other gods on walls;
>
> (v) A purely Brahma shrine has figures of Brahma in the principal niches round the shrine.

This conclusion indicates that there were no hard and fast rules for the orientation of a shrine, but generally the east was preferred. Ganapati had come to occupy, at least in Gujarat, the position he occupies now, the position of an auspicious deity, *Mangalamurti* or *Vighnaharta* (Remover of obstacles) and as such was placed in the centre of the door lintel in almost every temple. And this, in no way, indicates (or indicated as Burgess and Cousens thought) that the temple was dedicated to Siva. (Sankalia 1941 : 139).

Likewise an attempt was made to explain the socio-religious significance of the growth of the *mandapa* (main assembly hall) of the temples in India. It was pointed out that the growth of the institution of Devadasi (temple-dancer) seems to have been non-existent before the 4th-5th century A.D., and then its growth marched with the expansion of the *mandapa*, when in the mediaeval period in several parts of India, there were several hundreds of *devadasi* attached to the small and big temples. For this we have documentary proof.

Enough has been said above of what we can and cannot do to make archaeology in India a scientific discipline, to make it more an intellectual study rather than a simple discovery and description of objects. However, what is often not remembered and not practised , even if known, is that whatever we do must be well-planned, and scrupulously executed. Thus even without excavation, sites of the Painted Grey Ware, and megaliths, for instance, might be more intelligently plotted and some meaningful knowledge derived. The Painted Grey Wares' wide distribution extending from Northern Rajasthan and Eastern Punjab to Bihar-Nepal border in the east, and probably to Ujjain in the south is fairly well attested. This "coarse-grained" distribution map should now be replaced by a series of smaller maps where, in the initial talukawise distribution, sufficient attention has been paid to :

(i) the size of each site (as can be made out by surface indication),

(ii) its relation to bigger towns of the historic period,

(iii) its relation to the geographical features, and

(iv) its relation to the trade routes.

Thus, even without excavation wc shall be in a position to form a *more accurate* model/hypothesis about how the PGW sites stand in a parental relation to the subsequent development of Northern Rajasthan, Punjab and Uttar Psadesh. Such an investigation can well be made a part of the 5-year project in which several northern universities might cooperate.

Similarly megaliths in Maharashtra, Eastern as well as Western, Karnataka, Andhra, and Tamil Nadu should be intelligently plotted. Here Dr. A. Sundara (1974) has shown the way. Then instead of excavating one or two here and there, small and big one forming a megalithic field—for instance, we counted 74 at Edakkal Hills in Kerala—should be carefully excavated and *variability* in the *contents noted* and *accounted for*. Explanation must also be sought for the existence of a large number and the habitation site of the period should be searched. One idea with which I am playing with is "Do the megaliths represent the burials of a ruling or aristocratic class ? Otherwise who could afford so much wealth (whatever may be its form) to have such burial monuments" ? Of course, I should tell you that this point of view is not quite new. The late Professor Gordon Childe (1958 b : 13) had made a similar suggestion but opposed the commonly accepted view that the richness of a tomb offerings must reflect the general level of wealth or poverty. (Childe 1944 : 85, 87–8; 1945 : 13-19).

The archaeological interpretation of funerary remains, however, is not easy. In fact, Peter J. Ucko has recently shown, by citing numerous ethnographic parallels from Africa, Australia, as well as from a study of ancient literature and archaeological records, "that only with the widest possible approach to the archaeological material is it possible to appreciate the full significance of the excavated remains . . . . Variety and variability is a factor of the ethnographic funerary record which contrasts strangely with some of the archaeological assumptions about burial practices."

Again, has the orientation of the megalithic monuments in

India anything to do with the movements of the sun and moon as Professor Thom has shown by his study of the monuments in England ?

This survey of the work done hitherto in Indian archaeology, largely based on our experience at the Deccan College, would amply show that

1. there is certainly scope for the adoption of new techniques (not so much of excavation) as of data collection;
2. the prime need is of more and more horizontal excavations. These alone will give us data for socio-economic, religious and perhaps political organization during the Chalcolithic period in various parts of India;
3. these excavations should not be lightly undertaken (as done by some Universities and even the Survey);
4. excavation by proxy as well as study of the finds after years have to be given up;
5. adequate arrangements have to be made for on the spot study or "feed back";
6. the results of such a study might or should be tested by modern analytical methods and techniques;
7. such a study involves a multi-purpose organization—an expert each for botanical, zoological and soil studies attached to the expedition. Even C–14 dates should be had within two or three months of their collection. Secondly these should be immediately put in the space–time context of the excavated area.

If these things are done, and followed up by parallel historical and ethnographic studies, then much more meaningful knowledge about the past can be had than hitherto. But this knowledge would always be *approximate*. We can never hope to know and reconstruct the past fully and completely. Incompleteness is inherent in the subject itself. In spite of almost complete data about Tut-ankh-amen and his times, so

scrupulously recovered by Howard Carter and his team, doubts exist about several important points.

Ethnography & Archaeology in India

These lectures are being given under the auspices of the Ethnographic & Folk Culture Society and as I have pointed out, ethnography has played an important part in enlightening the archaeologist about the significance of the object he finds. But it is little realized that this is much more so in India than anywhere in the world ! For here in India, the past has never been completely divorced from the present, as in countries such as Greece, Rome, or Egypt, Mesopotamia, Turkey, even Iran, and of course Europe, whereas in Africa, Australia and America, the past continued far into the present until the other day, and the latter is so different that it does not require any scholar to distinguish the two cultural traditions.

But in India, the past is ever living, and one does not know how a knowledge of the life in the villages, or acquaintance with the lifeways of the primitives or our beliefs and superstitions, our religious practices (even in cities) coming down from Vedic times and many other sources might help you to explain the significance or function of an object you find in the excavation, or a sculpture you see on a monument. The more alert one's mind is, the greater the advantage one has in clothing with flesh and blood the dead bones of archaeology, So before ending this lecture, I will illustrate my point by a few examples so that the students of ethnography and archaeology as well as scholars and laymen interested in this subject can look for similar parallels when the occasion arises.

Sitala : Goddess of Small Pox

Years ago, when I was studying the temples of Gujarat, I noticed in one of the sculptures of a 12th century temple--perhaps Modhera in North Gujarat—a female figure—probably nude—riding on a donkey—and carrying a flat squarish object on her head. Nothing like this had been seen before, but Burgess said that it was probably the Sitala Devi (Goddess of

Small Pox). I was immediately reminded of the frequent scoldings my mother gave us, when by mistake or in play we held the winnowing basket (*Supa*, or *Supada*) on our head ! For, such an action done even in play was supposed to cause small-pox to the person ! Now this sculpture enables us to date the superstition in Gujarat at least to the 12th century A. D. Now, scholars interested in this folklore should tell us whether similar superstition exists, say in U. P., Bihar, Bengal, Punjab, Madhya Pradesh and other parts of India, and whether they have any figures of this nature on temples—ancient or modern. Secondly, we have to find out the real reason behind this superstition and its portrayal on a 12th century temple in Gujarat.

Channel-spouted bowl and Vedi

In our excavations at Navdatoli, among the several interesting things we discovered, two things are always before our mind.

One is a pottery vessel, called channel-spouted bowl; the other is a squarish pit, about 2 ft. square, sunk into the floor of the earliest floor of the habitation, and its borders as well as the floor smoothed with earth and cowdung. This channel spouted bowl always reminds me of a vessel which we use while performing a *tarpana* and such religious functions as *shraddha*, or a sacrifice, wherein the oblation–ghee or milk–is to be slowly and carefully poured on the *pinda*, or into the sacrificial fire. The latter is always kept in a *Vedi*, a squarish, specially built or made pit, its size dependent upon the place where it is to be performed. In a small house in a one or two room hut, it could not be more than 2 ft. square, but in a place like Sakori, where such sacrifices are regularly performed this pit is at least 6 to 8 ft. wide and at least 4 ft. deep. The question is, "Is the specially made pit at Navdatoli a sacrificial one ? and is the channel–spouted bowl a sacrificial vessel ?" Unfortunately, we cannot be definite, for the evidence is not cent per cent of that nature, though I personally believe that my surmise or hunch is right. But for asserting this state-

ment or proving this hunch, at least the two—the vessel and the pit—should have been found together, or the former, at least near the pit. Though we did not find the channel-spouted vessel either near or in the pit, we did find two other high necked vessels—with corrugated shoulders—in the pit, along with two burnt logs of wood. Thus there is little doubt that this pit was not an ordinary fire pit. However, if we could go one step further, and say that it was a *Vedi*, a sacrificial pit, then this one assertion—would change the whole complexion of the Navdatoli culture. It could belong to one of the Aryan tribes mentioned in the Vedas and Puranas. I would here rule out the possibility of this pit and the vessel belonging to some primitive people like the Bhil, who once occupied this region, and still form a majority in Gujarat and Madhya Pradesh. For a study of their material culture by Dr. (Miss) Malti Nagar has not given any indication that the Bhil use such a vessel today or could have used it in the past, some 3500 years ago. However, before pressing my "epoch-making" personal point of view, I would await more specific evidence from the future excavations at Navdatoli or anywhere in Madhya Pradesh, where such channel-spouted vessels are found. For, as I have been feeling and as Professor Binford has been emphasising, ethnographical parallel, to be of value, has to be proved or established.

Perforated pottery vessel

Like the channel-spouted bowl, the Southern Neolithic has sherds of shallow-bodied bowls with a few perforations confined to the central portion of the base, though in some cases, all over the body. The exact purpose of such a vessel could not be understood, until Dr. Paddayya drew our attention to a similar vessel used in Pamulapadu, Guntur District, Andhra Pradesh. (Paddayya 1969 : 450–53). The perforated vessel is a shallow round-bottomed bowl having a diameter of abuot 20 cm. and a depth of 6 cm. The bold collar below the edge of the mouth serves as a "hold-on" for the bowl. Now this vessel is used in the preparation of a specialized dish involving

cereal paste and milk. Briefly, the plastic cereal paste is pushed through the perforations so that macaroni-like tubular pieces are formed and cooked in the boiling milk. The dish is called *palalalikalu* (milk tubes). Paddayya has also argued that this was probably a Neolithic practice with plenty of milk in the diet and the (likely) occurrence of *jwar* and *ragi* as cereals in the Neolithic settlements at Kudutani and Hallur.

This important ethno-archaeological finding can be firmly established by the observation of the preparation of this dish all over South India, and the discovery of *ragi* / *jwari* or even rice grains in other Neolithic settlements of the south.

Ganesa

In this search for discovering the roots of our present beliefs, superstitions and religious practices, current excavations at Inamgaon have given excellent evidence. During the first or second season, we found a small clay male figure. This is normally regarded as a children's toy or a figurine, of not much importance. Imagine our surprise (and joy), when our labourers, all from the two local villages, Inamgaon and a village on the opposite bank, started preparing an identical clay figure at a communal feast organized when a foreign participant in the excavation gave some money to the labourers. So what we had found was probably a local deity, worshipped from prehistoric times, but not knowing its name, called it Ganesha! Thus, here is an interesting example where a prehistoric deity is endowed by the present day people of Inamgaon with the functions of a deity of the Puranic or early historic period, viz., as *Mangala Murti* and *Vighnaharta*. How far this attribution is right, we shall perhaps never be able to prove or establish.

Worship of goddesses

Still more interesting and significant was the discovery of a small clay box, hardly 6 in. long, 3 in. broad, and 2 in. high. This was no ordinary box. The lid, though crudely made, fitted well, with an overhang at each end. Inside there was

placed a nude female clay figurine in a sleeping posture with a claystand and outside a similar figure but headless, and a bull. I was not present at the site when this discovery was made by my colleagues Dr. Z.D. Ansari and Dr. M.K. Dhavlikar. But when I went to Inamgaon soon afterwards and was shown the box and the figurines, I immediately concluded that both these female figurines were goddesses and *actually worshipped,* either every day or on fixed occasions during the year.

You may ask how I reached this conclusion. When I saw the box and the contents, I was immediately reminded of what I and my parents did (this practice is continued even today) in our small flat in Bombay. Each day, after bath we opened a small wicker or cane basket, called *zhanpi,* took out the framed or unframed pictures of Shri Govardhan Nathji—Krishna holding up the Govardhan and worshipped as such at Nathadwara, near Udaipur—kept them standing against the cushions, and after the *seva* (worship) was over, again kept the pictures as before in a sleeping posture. This practice is followed by literally millions of Hindus, of all walks of life, in all strata of society, all over India. For, very rarely, one has a room large enough where one can keep the deity which is to be worshipped every day everready for worship. And, even if a room is there, still in all the temples of Vallabha Sampradaya, the small, child figure of Krishna is always taken out every morning, bathed, dressed, served *bhoga* (food) and again laid to rest (sleep). Hence I imagined that this was the practice followed by the Inamgaon family whose house we had unearthed after 3200 years.

My inference that these female figurines, because they were kept so carefully in a box, were worshipped was soon proved. For, there is a very simple, but intelligent contrivance, to keep both the figurines standing while worshipping. The female figurine within the box was supplied with a clay ring so that it can stand erect, while the headless figure stood on the bull's back. To achieve this, a small (blind) hole is made in the navel of the female figure and a similar hole in the spine of the bull, so that with a small stick the former can be kept standing on the

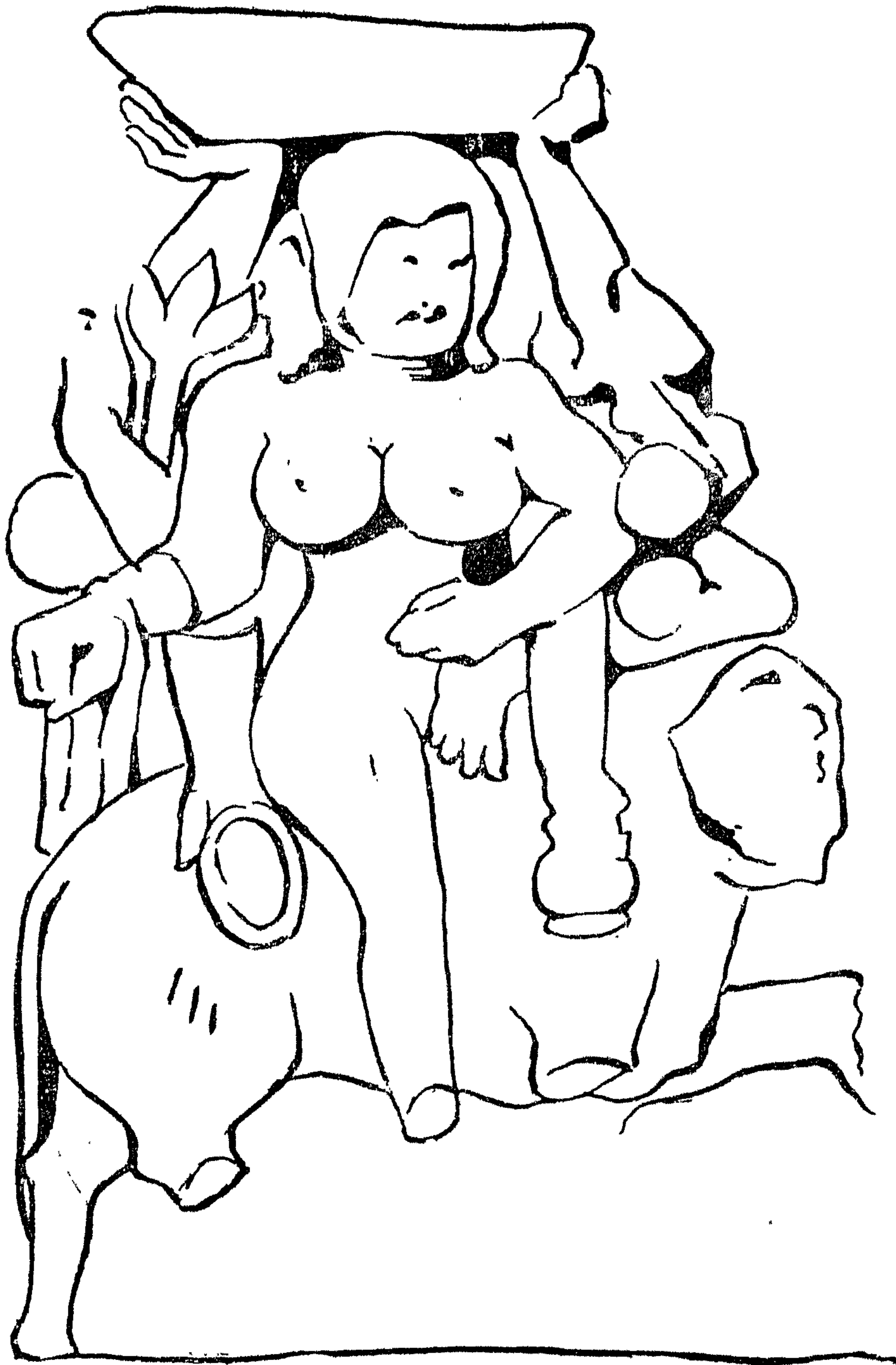


Fig. 8. Sītala (Goddess of Smallpox) seated nude on a donkey carrying a winnowing basket on her head. The original sculpture is on the Sun temple at Modhera, N. Gujarat, Early 12th century A. D.

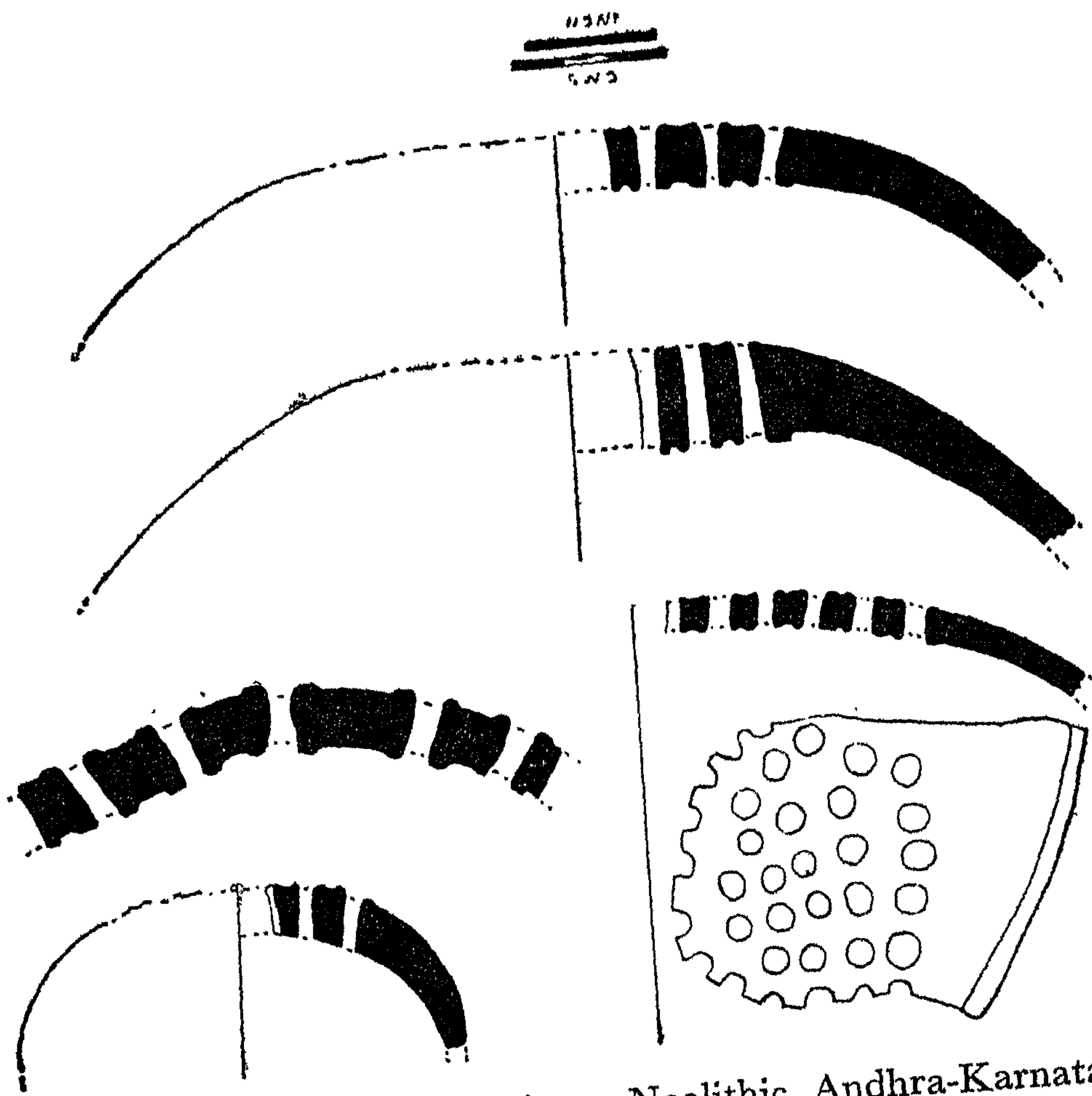


Fig. 9. Bowl with perforations, Neolithic, Andhra-Karnatak, S. India

[p. 94

bull's back. In fact, this is the earliest example in India of a deity with a *vahana* which became very popular in the early historic period. Thus, as the New Archaeologist wants or insists, we find evidence in the archaeological record itself at Inamgaon, for making a general statement that these female figurines with head, and without head but seated on a bull, were worshipped. From the evidence at Nevasa, we can also enlarge this conclusion that this was probably the goddess of the Jorwe Culture all over Western Maharashtra. My further inference that these goddesses were probably derived from Western Asia might be frowned upon by those who do not like such distant relationships based on diffusion, (the exact agency—trade, or migration of of people—being hitherto unknown), but in the absence of specific evidence from the intermediate countries, this inference is the only possible one if we plot all the evidence, as I have done, on a map. By all the evidence, or total evidence, I mean the evidence of domestication of animals, and domestication of plants and their dispersal. Such evidence as mentioned here, particularly the zoomorphic bull from Nevasa, Chandoli, and Nuzi in Iraq, and some pot forms, and designs, indicate a definite trend of things pointing to Western Asia as one of the chief sources from which various elements of civilization seemed to have radiated to India from about 5000 B.C. and have continued to do so through the early, middle and late historical times.

Enough has been said in these lectures to prove that archaeology has no longer remained a treasure-hunting, or search for mere objects or things. It is increasingly taking the help of sciences—all sciences, not only geology and palaeontology. Amongst these, anthropology and ethnography have a special role to play, for these alone help the archaeologist to enliven the past. Of course, these disciplines are to be used with care. And with the help of statistics and computers, we can make all our observations more objective, and impersonal to a degree. Still these do not and cannot make archaeology fully scientific, for imperfection or incompleteness is rooted in its nature and constitution. Trigger has so well said ; "Text-free archaoelogy

must remain what the New Archaeology has not fully convincingly denied it need be : a fragmentary basis for the study of human behaviour". (Trigger in Adams 1974). This alone lends it a charm and draws the illiterate as well as literate towards it.

India is a living museum and offers an excellent opportunity for practising all these manifold approaches, techniques and methods, either singly or jointly. What we need above all is trained and devoted workers, some vision and a definite plan. All these the late Dr. D.N. Majumdar exemplified in himself to the highest degree. Let us therefore hope that this modest offering to his memory will inspire our students and colleagues to come forward, adopt newer methods and techniques, and refine them further so that we know as much of Man as possible.

● ●

APPENDIX I

New Archaeology

Later Dr. Bruce Trigger, Professor of Anthropology at McGill University, Montreal, briefly reviewed all the important viewpoints—orthodox or conservative as well as heterodox, or advanced—and came to the conclusion that while Jacquetta Hawkes had misinterpreted the nature of historical enquiry as it is understood by modern historians, her condemnation of natural science methods might be regarded as a criticism of all prehistoric archaeology—including the current developments—since the days of Christian Thomsen. He also points out how in Binfords' use of deductive methodology as generalizing and rejection of the inductive as particularizing, one loses the concept of history. He then concludes, "prehistoric archaeology has an important role to play as an historical discipline within the larger framework of the science of man. Such a definition includes an interest in process as well as in events and chronology. The aim of any historical discipline is not only to describe, but to interpret specific events....By attempting to understand and explain the past, archaeologists are contributing to human awareness." (Trigger 1970 : 26-36).

Professor Richard A. Watson of the Department of Philosophy, Washington University, St. Louis Missouri, briefly reviewed the entire development, particularly in America, and generally approved of the new approaches of Binford and his colleagues.

He concluded by saying that the new archaeology has been primarily an American concern, that structurally we have seen

it all before in the physical and then in biological sciences. There is an initial concern with categorization and taxonomy, then with explanation and prediction based on lawlike generalizations. Now it is happening in the social sciences, at one extreme of which stands archaeology....The explicit awareness of and application of the hypothetic-deductive method makes legitimate the old claims that archaeology can make a unique contribution to the generalized understanding of human culture. (Watson 1972 : 210-14).

Walter thinks that Binfords' *NP* in *A*. contains clear statements of the explicitly scientific method of work guided by this approach. And he cites the excellent articles of Flannery, Longacre, Strucwer and Whallon as good examples of scientific archaeology. (p. 214).

A. C. Hogarth who is a teacher in England, and also an excavator, said, "there is no such thing as the New Archaeology. It is merely Newspeak Archaeology, tricked out in a whole wardrobe of new vocabulary apparently designed more to impress than to enlighten." However, Hogarth does realize the various analytical approaches discussed by David Clarke, though he warns us in regarding the computer as a sort of super-nanny which will understand, care for and work for the archaeologist; and that of others to talk to a human audience in the same electronic baby-talk which is necessary for addressing a computer. "These aides no more make archaeology a 'science' than a wooden leg makes a man into a tree. Mathematical aids cannot make it a mathematical discipline."

A. is and should remain a bridge across the great gulf between the sciences and the humanities. A. cannot and should not be identified with one or the other side of the "snow" line. (Hogarth 1972 : 301-04).

APPENDIX II

Millie's Camp

Millie's camp is a recently-abandoned Indian camp-site in the central Canadian Rockies near the new coal-mining town of Grande Cache, Alberta. While camping in the area in 1968 an acquaintance was made with Millie, one of the previous occupants of the camp, through a fortuitous meeting. It was realized that Millie and her former camp could provide important components in an experiment in archaeological methodology.

The experiment presented in this study sets out to test the validity of the intuitive method of inferential reasoning. Millie's Camp, although recently abandoned, is treated as an archaeological site. The investigation undertaken is based on the proposition that the structured distribution of features and artefacts reflect aspects of behaviour and organization of the group who occupied the camp. Following the lead set by Longacre and Ayres (1968) in their article, 'Archaeological lessons from an Apache wickiup', an attempt is made to test the validity of interpretations by consulting an informant—Millie, one of the former camp occupants.

In analysis of this site the 'activity area' concept has been used to isolate organizational units in the camp. The term 'activity area' as used here denotes three kinds of areas; included are features, features and associated artefacts and associated clusters of artefacts. After the activity areas were identified a scale plan-view map was drawn. Each area was assigned an activity area number.

Conclusions

Millie's abandoned camp has provided an experimental vehicle for making a preliminary evaluation of the utility of the intuitive analytic approach. First, a cultural reconstruction based on intuitively-derived interpretations was made on the recently-abandoned camp. Then, these interpretations were subsequently tested for validity by consulting Millie, one of the former occupants of the camp. Several major kinds of inferential errors were revealed as a consequence of this avenue of research.

1. Items were misidentified and assigned to the wrong functional categories.
2. False associations were made between items.
3. Activity areas were interpreted incorrectly.
4. The relationships between activity areas were misinterpreted.

A sequence of errors such as these reflect the fact that mistakes made at the identification level are frequently carried over into each succeeding higher level of interpretation. For example, a misidentified artefact can influence the interpretation assigned to an activity area, which in turn may lead to an incorrect interpretation of the relations between activity areas. Hence, a single error may be compounded several times, affecting the overall accuracy of a proposed reconstruction.

In this case a shift in camp design to facilitate the complex of activities affiliated with deer butchering and processing resulted in the duplication of hearths and refuse areas used by the family. Of interest is the fact that this complex of related activities cross-cut other activity areas, and particular work tasks were conducted at individual features.

This point indicates that a synchronic model such as the one employed here should be dropped in favour of an approach that takes into consideration the temporal and spatial dimension of activity areas.

The present preliminary study serves to underline the fact that the intuitive analytic approach commonly used for the inter-

pretation of prehistoric remains should be critically examined. As this approach lacks empirical referents, the basic prerequisite for a comparative science, it is unlikely that any two investigators examining the same data will generate similar conclusions. It seems unlikely that the analysis of material culure can ever become a comparative science until investigators make explicit their analogue sources. Considerable emphasis must be placed on developing and testing forms of analogue reasoning based on contemporary practices that can be used for interpretative purposes. Old line ethnographers did not record many kinds of information necessary to answer the kinds of systemic questions present-day historians are raising about the relationships between material remains and other aspects of culture. If these questions are to be answered it is up to the archaeologist to develop his own interpretative framework for evaluating prehistoric data.

In conclusion, this study suggests that although the prehistorian may be able to develop logical, satisfying explanatory structure for understanding prehistoric data, there need not be any relationships between his model and the site under investigation. If difficulties such as those encountered here are to be curtailed in the future an acute awareness must be developed of limitations of inferential methodologies employed for interpreting prehistoric remains.

BIBLIOGRAPHY

Adams, Robert McC. 1974 Anthropological perspectives on ancient trade. *Current Anthropology* 15 : 239-58.

Adams, William Y. 1968 Invasion, diffusion, evolution? *Antiquity* XLII : 194-215.

Atkinson, R. J. C. 1968 Review of Alexander Thom. 'Megalithic sites in Britain'. Oxford 1967 in *Antiquity* XLII: 77-78.

Binford, L. R. and S. R. Binford 1966 A preliminary analysis of functional variability in the Mousterian of Levallois facies. *American Anthropologist* 68.2 : 238-95.

Binford, S. R. and L. R. Binford 1969 Stone tools and human behaviour. *Scientific American* 220, 4 : 70-84.

1970 *New perspectives in Archaeology*. Chicago.

Binford, S. R. 1970 Variability and change in the Near Eastern Mousterian of Levallois facies. in *New perspectives in archaeology*. eds. S. R. and L. R. Binford, Chicago.

1972 *An archaeological perspective*. New York and London.

Bonnischen, Robson 1973 Millies' camp, an experiment in archaeology. *World Archaeology* 4 : 277-291.

Bordes, F. 1959 Evolution in the palaeolithic culture. in *Evolution after Darwin: The Evolution of Man.* ed. Sol Tax. Chicago.

1961 *Typologie du Paleolithique Ancien et Moyen.* Bordeaux.

1968 *The old stone age.* London.

Bordes, Francois and Denise de Sonneville Bordes 1970 The significance of variability in palaeolithic assemblages. *World Archaeology* 2 : 61-73.

Bordes, Francois 1971 Physical evolution and technological evolution in man : A parallelism. *World Archaeology* 3 (1) : 1-3.

Boserup, E. 1965 *The conditions of agricultural growth.* London.

Bowler, J. M., Jones Rhys, Allen Henry and A. G. Thorne 1970 Pleistocene human remains from Australia : a living site and human cremation from Lake Mungo. Western New South Wales. *World Archaeology* 2, 39-60.

Braidwood, R. J. 1960 The agricultural revolution. *Scientific American* 203 : 130-48.

Braidwood, R. J. and B. Howe 1960 *Prehistoric investigation in Iraqi Kurdistan.* University of Chicago.

Brothwell, Don R. and Eric Higgs (eds). 1963 *Science and Archaeology.* London.

Brothwell, Don and Patricia 1969 *Food in Antiquity.* London.

Chang, Kwan-Chih. n.d. Settlement patterns in Archaeology. Addison-Wesley Publishing Co. Mass.

Childe, V. G. 1944 *Progress and Archaeology.* London.

1944 Archaeological ages as technological stage. *Journal of the Royal Anthropological Institute.*

1945 Directional change in funerary practices during 50,000 years. *Man* 4 : 13-19.

1956 *Piecing together the past : the interpretation of archaeological data.* London.

1958a Valediction. *Bull. Inst. Archaeology* I, 1 ff.

1958b *The prehistory of European society.* London.

Chitalwada, Y. M. 1974 Settlement pattern and analytical assessment of the concept. *Prachya Pratibha* II. Journal of the Birla Institute, Bhopal.

1967 *Cambridge Ancient History.*

Clark, J. D. and C. U. Haynes, Jr. 1970 An elephant butchery site at Mwangands village, Karonga, Malawi and its relevance to palaeolithic archaeology. *World Archaeology* I : 390-411.

Clark, J. Desmond n.d. *The cultural life of early man.* Addison Wesley Publications Co. Mass.

Clark, J. G. D. 1952 *Prehistoric Europe : the economic basis.* Cambridge, University Press.

1954 *Excavation at Starr Carr.* Cambridge.

1957 *Archaeology and society.* London.

Clark, J. G. D. and S. Piggot 1965 *Prehistoric societies.* London.

Clark, J. G. D. 1966 The invasion hypothesis in British archaeology. *Antiquity* XL : 172-189.

1972 *Starr Carr : A case study in bioarchaeology.* Addison–Wesley Modullar Publications Module 10 : 1-42.

1973 Bioarchaeology : some extracts on the theme. *Current Anthropology* 14 : 464-470.

Clarke, David L. 1968 *Analytical archaeology.* London.

1973 Archaeology : the loss of innocence. *Antiquity* XLVII : 6-18.

Collins, D. M. 1969 Cultural traditions and environment of early man. *Current Anthropology* 11 : 267-316.

Collins, Desmond 1970 Stone artefact analysis and recognition of cultural traditions. *World Archaeology* 2 : 17-27.

Corvinus-Karve, G. 1967 An Acheulian occupation floor at Chirki on Pravara. *Current Anthropology* 9 : 216-218.

1970 The Acheulian workshop at Chirki on Pravara, Maharashtra. *Indian Antiquary* 4 : 13-22.

Daniel, Glyn E. 1943 *The three ages.*

1950 *A hundred years of archaeology.* London.

1973 *Antiquity* XLVII : 93 and 175.

Deo, S. B. 1970 *Excavations at Takalghat and Khapa.* Nagpur.

1973 *Mahurjhari excavation 1970-72.* Nagpur.

Dhavalikar, M. K.	1970	A prehistoric deity of Western India. *Man* 5 : 131-132.
Dhavalikar, M. K. and G. Possehl	1974	Subsistence pattern of an early farming community of Western India. *Puratatava* 7 : 39-46.
Freeman, L. G.	1972	in "the Pan American Congress of Prehistory and the Study of Quaternary". Bulletin 5. Berkeley (cyclostyled).
Goodall, J.	1964	Tool-using and aim in throwing in a community of free loving chimpanzees. *Nature* 201 : 1264-66.
Harlan, Jack R.	1971	Agricultural origins : centres and non-centres. *Science* 174 : 468-74.
	1971	The evolution of cultivated plants. in *Genetic resources in plants : their exploration and conservation.* eds. O. H. Frankel and E. Bennett : 19-32.
Harris, James E. and Kent R. Weeks	1973	*X-Raying the Pharaohs*. London.
Hawkes, J.	1968	The proper study of mankind. *Antiquity* XLII : 255-62.
Higgs, E. S. and M. R. Jarman	1969	The origins of agriculture : a reconsideration. *Antiquity* XLIII : 31-41.
Higham, Charles F. W.	1969	Towards an economic prehistory of Europe. *Current Anthropology* 10 : 139-150.
Hodson, F. R.	1970	Cluster analysis and archaeology : some new developments and applications. *World Archaeology* I : 299-320.

[See also Leroy Johnson. Jr. Bulletin no. 15 Museum of Natural History, University of Oregon, and BPS July 1971 XXXVII : 231-33].

Hogarth, A. C. 1972 Common sense in archaeology. *Antiquity* XLVI : 301-04.

Hole, Frank and Shaw Mary n.d. "Computer analysis of chronological seriation". (Rice University Studies, 53, no. 3). Houston, Texas.

Hole, Frank and Robert F. Heizer n.d. *An introduction to prehistoric archaeology.* (Third Edition). New York.

Hoyle, Hawkins, Thom 1967 in "Hoyle on Stonehenge. some comments". *Antiquity* XLI : 91-98.

Jarman, Micael 1971 Culture and economy in the North Italian Neolithic. *World Archaeology* 2 : 255-65.

Joshi, J. P. 1972 Explorations at Surkotada and new light on the Harappan migration. *Journal of Oriental Institute* 22 : 92-144.

Joshi, R. V. 1964 Achaeulian successions in Central India. *Asian Perspective* 8 : 150-63.

Lal, B. B. and B. K. Thapar 1967 Excavations at Kalibangan. *Cultural Forum* 34 : 78-88.

Lawick-Goodall, J. Van 1967 *My friends the wild chimpanzees.* Washington, *National Geographic Society.*

Leakey, Richard 1973 Skull 1470. *National Geographic* 143 : 820.

Longacre, William A. 1970 Some aspects of prehistoric society in East-Central Arizona. in *New Perspectives in Archaeology*

eds. Sally R. & Lewis R. Binford : 89-102.

Mackay, Ernest J. H. 1938 *Further Excavations at Mohenjodaro.* 2 Vols. New Delhi.

1943 *Chanhu-daro Excavations.* New Haven.

MacLeish, Kenneth 1972 Stone age caveman of Mindano. *National Geographic* 142 : 219-248.

Malik, S. C. 1968 *Indian civilization : The formative period.* Simla.

Marshakck, Alexander 1972 Upper palaeolithic notation and symbol. *Science* 178 : 817-828.

Marshall, John 1931 *Mohenjodaro and the Indus civilization.* 3 Vols. London.

Mellars, Paul 1970 Some comments on the function of 'functional variability' in stone tool assemblages. *World Archaeology* 2 : 74-89.

Misra, V. N. 1973 Bagor—a late mesolithic settlement in northwest India. *World Archaeology* 5 : 92-110.

Morgan, C. G. 1973 Archaeology and explanation. *World Archaeology* 4 : 259-276.

Movius, Hallam L. 1968 Segmented backed bladelets. *Quarter* 19 : 239-49.

Movius, Hallam, L. Jr. et al 1968 The analysis of certain major classes of upper palaeolithic tools. *American School of Prehistoric Research, Peabody Museum, Harvard University Bulletin 26.*

Nagar, Malti 1966 *The Ahar culture : an archaeological and ethnographic study.* Ph. D. thesis (Unpublished). Poona University and Deccan College Libraries.

Nagaraja Rao, M. S. and K. C. Malhotra 1965 The stone age hill dwellers of Tekkalakota. Poona.

Oakley, Kenneth, P. 1969 Man the skilled toolmaker. *Antiquity* XLIII : 222-23.

Olsen, S. J. n.d. *Zooarchaeology : animal bones in archaeology and their interpretation.* Addison–Wesley Publishing Co. Mass.

Paddayya, K. 1969 Forms and functions of the perforated pottery of the Deccan Neolithic Culture. *Man* 4 : 450-452.

"*The Pan African Congress of Prehistory and the Study of Quaternary*" Berkeley, 1972. (Cyclostyled).

Pfeiffer, J. E. 1969 *The emergence of man.* New York and London.

Porter, James, W. n.d. *Late Analysis of cerematic materials.* Addison Wesley Publishing Co. Mass.

Rao, S. R. 1973 *Lothal and the Indus civilization.* Bombay.

Reddy, V. R. 1968 *Pre- and proto-history of the South Western Andhra Pradesh.* Ph. D. Thesis (unpublished). Deccan College and Poona University Libraries.

Redman, Charles, L. (ed). 1973 *Research and theory in Current Archaeology.* New York and London.

1974 *Archaeological sampling strategies.* Addison-Wesley Publishing Co. Readings. Mass.

Renfrew, Colin 1968 Models in prehistory. *Antiquity* XLII : 132-34,

1969 Trade and culture process in European prehistory. *Current Anthropology* 10 : 151-169.

1970 New configurations in Old World archaeology. *World Archaeology* 2 : 199-211.

Sankalia, H. D.

1941 *Archaeology of Gujarat.* Bombay.

1965 *Excavations at Langhnaj, 1944-63 Part I : Archaeology.* Poona.

1966a Prehistoric men and primitives in South Gujarat and Konkan. *Journal of Anthropological Society of Bombay* 12 : 34-38.

1966b Socio-economic and geographical background of Khajuraho. *Journal of Indian History* XLIV, 130.

1967 Socio-economic significance of the lithic blade industry from Navadatoli, Central India. *Current Anthropology* 8 : 262-68.

1970 The human and functional approch in archaeology. *Proceedings of the 57th Indian Science Congress.* Presidential address, Part II. Khargapur.

1972 The earliest private shrine in Western India and its relationship with the Mother Goddess in Europe and Western Asia. in *Summary of papers : Symposium International Sur Les Religions de la Prehistorie.* Valcomonica, Italy.

1972-73 The Cemetry-H culture. *Puratattva* 6 : 12-19.

1973 Prehistoric colonization in India. *World Archaeology* 5 : 86-91.

1974a *Prehistory and protohistary of India and Pakistan.* Poona.

1974b Cultural traditions *versus* functional activities in stone age and other archaeological assemblages, in *N. Ray Felicitation Volume.*

1974c Functional significance of the O. C. P. and P. G. W. shapes and associated objects. *Puratattva* 7 : 47-52.

Sankalia, H.D., S.B. Deo and Z. D. Ansari 1960 *From histvry to prehistory at Nevasa.* Poona.

1969 Excavations at Ahar. Poona.

Sankalia, H.D., S.B. Deo and Z. D. Ansari 1971 *Chaleolithic Navdatoli.* Poona, Baroda.

Semenow, S. A. 1964 *Prehistoric technology.* Bath.

Singer, Ronal, John Wymer, Bruce G. Gladfelter & Ronal G. Wolff 1973 Excavation of the Clactonian Industry at the Gold Course, Clacton-on-Sea, Essex. *Proceedings of the Prehistoric Society* 39 : 6-74.

Smith, C. Earle, Jr. 1968 The New World centres of origion of cultivated plants and the archaeological evidence. *Economic Botany* 22 : 253-66.

Sonneville-Bordes, Denise de and Parrot Jean 1953 Essai d' adaption methodes statisques an paleolithique superiecer—Premiers resultats. *Bulletin de la Societe Prehistorique Francaise* I 50, 5-6 : 323-333.

Spaulding, Albert C. 1953 Statistical techniques for the discovery of artifact types. *American Antiquity* 18 : 305-313.

Statistical description and comparison of artifact assemblages. in *The application of quantitative methods in archaeology.* Viking Fund Publication 28 : 60-83.

1968 Explanation in archaeology. in *New perspectives in archaeology.* eds. Sally R. & Lewis R. Binford : 30-40.

Subba Rao, B. 1958 *The personality of India.* 2nd Edition. Baroda.

Trigger, B. G. 1968 *Beyond history : the methods af prehistory.* New York.

1970 Aims in prehistoric archaeology. *Antiquity* XLIV : 26-37.

Ucko, Peter J. n.d. *Evidence of religion from prehistoric remains.* Addison Wesley Publishing Co. Reading Mass.

1969-70 Ethnography and archaeological interpretation of funerary remains. *World Archaeology* I : 262-280.

Ucko, P. J. and G. W. Dimbley 1969 *The domestication and exploitation of plants and animals.* London.

Uerpmann, Hans-Peter 1973 Animal bone finds and economic archaeology : a critical study of osteoarchaeological method. *World Archaeology* 4 : 307-322.

Vats, M. S. 1940 *Excavations at Harappa.* 2 Vols.

Vavilov, N. I. 1926 *Studies on the origin of cultivated plants.* Leningrad.

1960 *Archaeological survey of the main field crops : Vol. 1. World resources of cereals, leguminous seed crops and flax.*

Vita-Finzi, C. and E. S. Higgs	1970	Prehistoric economy in the Mount Carmel area of Palestine :Site catchment analysis. *Proceedings of the Prehistoric Society* XXXVI : 1-37.
Walter, Richard A.	1972	The 'new archaeology' of the 1960s. *Antiquity* XLVI : 210-15.
Watson, Patty, Jo Steven, A. LeBlanc and Charles L. Redman	1971	*Explanation in archaeology : an explicitly scientific approach*, New York.

INDEX


CENTRAL ARCHAEOLOGICAL
New Delhi
LIBRARY

